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TELPHERAGE.

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From the "Journal of the Society of Arts."

In the first place, it is necessary that I should define what is meant by this word "telpherage," and perhaps that I should defend its formation. The word is intended to designate all modes of transport effected automatically with the aid of electricity. According to strict rules of derivation, the word would be "telephorage;" but in order to avoid confusion with "telephone," and to get rid of the double accent in one word, which is disagreeable to my ear, I have ventured to give the new word such a form as it might have received after a few centuries of usage by English tongues; and to substitute the English sounding "telpher" for "telephore."

In the most general sense, telpher lines include such electric railway lines as were first proposed by my colleagues, Messrs. Ayrton and Perry. The word would also describe lines such as I have seen proposed in the newspapers, for the conveyance of small parcels at extremely rapid rates. But to-night I shall confine myself entirely to the one specific form in which the telpher line first presented itself to my mind, and which it has fallen to my lot to develop. In this form telpher lines are adapted for the conveyance of minerals and other goods at a slow pace, and at a cheap rate.

The problem which occurred to me was this: Was it not really possible to send vehicles, by means of electricity, along a single suspended wire or rod—in fact, to telegraph goods and passengers instead of messages. The idea is familiar as a joke, but, on consideration, it appeared that there might be good grounds for supposing both that the idea was practicable and useful. I am now able to show you the realization of that idea, and the result of experiments on a large and practical scale has, I think, justified the arguments which have induced me to devote much time and labor to telpherage.

[Here the model was shown in action. This model consisted of two concentric octagons of wire, the length of each outer span being 5 ft. On each octagon there was a single locomotive and train, equal in length to that of the span. These trains ran well and steadily in opposite directions round the lines.]

These arguments may be stated as follows:

We could not, with steam, employ a vast number of little one-horse engines to pull along a number of small trains or single wagons. There would be waste in the production of power, and great cost in the wages of the men employed at each engine. But an electric current, of, let us say, 50 horse-power, will, as it circulates through a conductor of mod-

erate size, drive thirty small engines each of one horse-power, which require practically no supervision, and can be made nearly as economical in their action as a single electromotor of 30 horse-power could be.

But if the power can be distributed economically along a line, say, ten miles in length, this allows us to employ thirty small trains, corresponding each to a waggon pulled by one horse, instead of a single train such as might require 30 horse-power. It we further distribute the weight by making each train of considerable length, we are able to employ an extremely light form of road, such as a suspended rope or rod of, say, $\frac{3}{4}$in. diameter. Later on in the paper I will show the amount of traffic which such a rod can practically convey. Meanwhile, I simply draw your attention to the general principles of the subdivision of power and the subdivision of weights. In distributing the power by means of electricity, it was clear that considerable waste must be incurred, but the amount of that waste is easily calculated, and is by no means prohibitory. Moreover, the power, being obtained from stationary engines, or in certain cases from falls of water, could be produced at a cheap rate in comparison with that obtained from locomotives or traction engines.

When I examined the various forms of possible road by which the distributed power and distributed load could be conveyed, it seemed to me that the single suspended rope or rod offered great advantages. The smallest railway involved embankments, cuttings, and bridges, fencing, and the purchase of land. A single stiff rail, with numerous supports, from which the train might hang, seemed better, and may, in some cases, be employed, but the supports would require to be numerous—say, one post every 10 or 15 feet—and even with these spans, the girder required to carry vehicles weighing 2 cwt. each, would be costly. With a single suspended rod or rope, we may have supports 60 or 70 feet apart. A $\frac{3}{4}$-inch rod, thus supported, will carry five vehicles, each bearing 2 cwt., without excessive strain. No purchase of land is necessary, no bridges, earthworks, or fencing. The line can be so far removed from the ground that it will not be meddled with, either by men or animals. A single wheel-path gives the minimum of friction, and the rolling stock can be much more easily managed than if we attempted to let vehicles run on double swinging ropes. On all those grounds it seemed well worth while to devise means by which trains could be electrically and automatically driven along the single suspended rod.

Before proceeding further, I had better state how far this idea has been realized. The Telpherage Company, Limited, was formed last year, to test and carry out my patented inventions and those of Professors Ayrton and Perry for electric locomotion. On the estate of Mr. M. R. Pryor, of Weston, two telpher lines, on my plan, have been erected. One of these is a mere straight road, with spans of 60 feet, and various forms of rod and rope. The first full-sized train was run on this line with a locomotive which we call the bicycle-wheel loco (Figs. 8 and 9). The line was found inconveniently large and high, and the experiments were continued on a line $\frac{5}{8}$-inch diameter, of round steel rods, with 50 feet span. This line is continuous, that is to say, it re-enters on itself. It is 700 feet long, and we have run a train of more than one ton at a speed of five miles per hour on this line with complete success. The insulation has given no trouble. It need hardly be said that we see our way to great improvements in details. Thus, we can make the road more uniform, and stronger for its weight; we can lessen the quantity of material used, and greatly diminish the amount of skilled labor required in erection. We can improve the design of the posts. We can improve the trucks and locomotives, so that they will go around sharper angles, and so forth, but the main object has been practically carried out. We have had trains on a scale as large as I am prepared to recommend, running at the highest speed I have contemplated.

I trust it will be clear to you, from this description, that what I have contemplated and realized is not an electric railway destined to compete with steam railways in conveying goods and passengers at high speeds, neither is it a new form of communication destined for small parcels and high speeds; it is simply a cheap means of conveying heavy goods

which, like coal or grain can be carried in buckets or sacks, each containing two or three hundredweight. The speed on a telpher line will be that of a cart, and the object we aim at is to cart goods at a cheaper rate and more conveniently than with horses.

I assume that you all know that an electric motor is a machine which will run so as to exert power whenever an electric current is passed through it. You also know that a machine called a dynamo, driven by a steam-engine or other source of power, will produce an electric current which may be conveyed along a suspended and insulated rod, and used to drive an electric motor.

In describing the details of my system, the first point to be explained is, how the current produced by the dynamo, and conveyed along a single line, is taken from that line and directed round the motor.

In endeavoring to realize this idea, the first thought which occurred to me was that of dividing the line into lengths, equal to the length of the train, so that using the train to bridge over a gap between two sections at different potentials, the current could be conveyed from the leading to the trailing wheels of the train, round the motor. This idea is employed in the model now shown; but, in the first form which suggested itself, the gaps between the sections were opened by a switch worked by the front of the train, and closed by a switch worked by the end of the train. The first model, which may have been seen by some present, working in Fitzroy-street, was made on this plan. Trains driven in that way would all be coupled series. The present model is differently arranged; there are no working parts or switches. Let the successive sides of the polygon be called the odd and even sides; the odd outer sides are connected with the even inner sides, and the even outer sides with the odd inner sides. We thus have two continuous conductors each going right round the model, but not joined to each other; these are connected to the two poles of a battery. So long as no train bridges a gap no current flows, but whenever the train bridges the gap, a current flows from the positive to the negative pole round the motor. This plan is called the cross-over system; all the trains are joined by it in parallel arc, and the current is reversed each time a train passes a gap. This reversal does not affect the working of the motor. This is the plan which has been carried out on a large scale at Weston. Its simplicity leads me to believe that it will be the plan most usually adopted, but several other methods of driving have been devised. A spark passes between the wheels and the line each time the current is stopped, but this spark occurs between large masses of metal, where it appears to be harmless; it has given me no trouble whatever at Weston. Moreover, it has been found very easy to make connection between the line and the train. The ordinary truck wheels answer admirably, so that no complicated brushes are required. There are some absolute advantages in having interruptions at regular intervals, but the discussion of these would lead me too far for my present purpose.

Only one of the two continuous conductors requires to be insulated; this results in alternate insulated and uninsulated sections all along each line. Fig. 1 shows a saddle, as we call it, with an

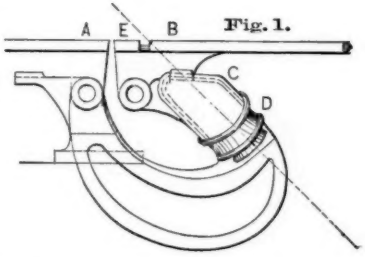


Fig. 1.

insulated attachment, B, at the one end, and an insulated attachment A at the other, as used for a short sample line which has just been sent to Peru for the Nitrates Railway Company, Limited. The line itself is a three-quarter inch steel rod with forged ends, and Fig. 2 sufficiently shows the mode of attachment. The insulation is given by a vulcanite bell insulator D, carrying a cast-iron cap C. All the parts are designed to stand 2.2 tons strain; the vulcanite is secured between two layers of Siemens' cement. The experiments at Weston have shown that vulcanite answers per-

fectly, but the material is rather expensive. I have here a smaller porcelain insulator, which has been subjected to 2.2 tons' strain. I believe porcelain will answer well in all respects, but it has not yet been subjected to the test of actual traffic day by day. At Weston the vulcanite was used between layers of Portland cement, the only objection to which is, that it takes some time to set. The simple steel rod has been found preferable in all ways to rope. We find that there is less friction and less jar with the rod, and ample flexibility; it is also much easier to secure. Moreover, a solid rod with welded ends can be made so that the ends, where supported, are, to some extent, undercut, as is shown in the corresponding bulb angle-iron (Fig. 3) used for rigid parts

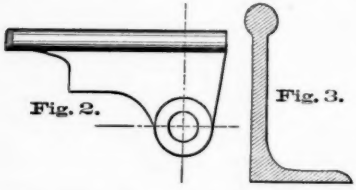


Fig. 2.

Fig. 3.

of the road; this undercutting allows much greater freedom of rolling than would be compatible with the horizontal gripping wheels, especially when gripping wheels are used which, like those in the model, actually hold on to the line so as to resist being lifted. A short piece E, slightly insulated, prevents the sections from being short-circuited by the wheels.

Fig. 4.

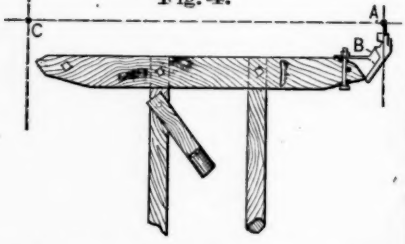
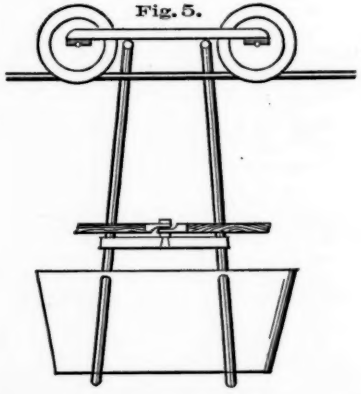


Fig. 4 shows the posts and crosshead supporting the line. In the one-inch example this design was fully carried out, and the posts stood the cross strain due to the overhanging load perfectly. In the five-eighth line an attempt was made to cheapen the construction, but the posts in wet weather work at the foundations; it is well that we are put on our guard against this danger. In the first design a sort of rocking saddle was employed, to allow the strain to be transmitted from one span to the next, but the flexibility of the posts provided amply for this object.

Abutment posts are required at intervals, and these can be made use of to provide compensation for changes of temperature, and to limit the stress on the rods. In straight lines I reckon about four abutment posts per mile.

In the short South American line, curves of 45 degrees at the posts will be employed, as shown in the model. At the stations where goods are to be handled, a rigid rod will be more convenient than the flexible rod. A bulb angle-iron like that shown in Fig. 3, supported every ten feet, answers well at Weston, and a siding, leading the trucks off this line, has been satisfactorily carried out. The siding leads back to the line at a point between two flexible spans. In fine, it may be said to-night that the problem of the continuous line, whether straight, curved, rigid, or flexible, has been completely solved. Drawings and specifications can be put, without further delay or experiment, into the hands of contractors.

Fig. 5.



Trucks used on ordinary rope lines are designed to be pulled by ropes on a road which is necessarily straight. When

trucks of this description, with wheels 8 in. diameter and 22 in. wheel-base (Fig. 5), were tried at Weston, arranged in trains, some new difficulties presented themselves. Any sudden check to the motion was followed by a rearing action, throwing the truck off the line; similar results followed the application of any sudden pull. Moreover, trucks with two rollers on a rigid frame, even with so great a wheel-base as 22 in., require curves of considerable radius if we are to avoid serious binding at the flanges. Notwithstanding these difficulties, the trains at Weston, with a little care, run well and lightly, but the trucks which have gone to South America are on the plan adopted in the model, and run much more safely, and turn much

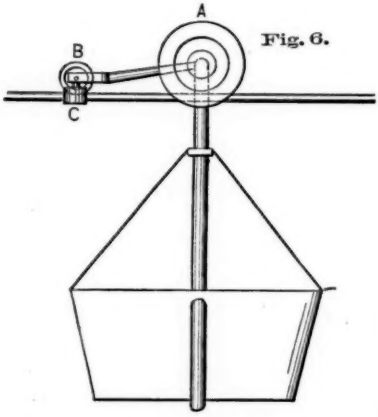


Fig. 6.

sharper curves. They have two peculiarities—first, each wheel 7 in. diameter (Fig. 7), is pivoted on an axis, B, vertically over the center of the wheels, A; this allows the truck to run with the freedom of a bicycle round curves; secondly, the weight carried is hung on a swinging arm, D, pivoted to the frame at a point, P, on a level with the line. The result is that any force applied in a plane containing the line acts as if applied at the line itself, and will neither lift the wheels in front nor behind. In the model, the coupling, as you see, is on one line attached to the top of the swinging arm, where the coupling rods are well out of the way. In the other line the coupling is below the rod. The swinging arm relieves the locomotive from all jerk at stopping or starting.

The truck is completed by a small hook or catch embracing the rod. In case of any accident causing the wheels to leave the line, this hook will prevent the truck from falling. The weight of the two-wheel stiff truck, shown in Fig. 5, with wrought-iron buckets, is 75 lbs. The weight of the two-wheel pivoted trucks, with wooden bucket, is 63 lbs. They are both adapted to carry 2 cwt. Fig. 6 shows a one-wheeled truck tested—the results were not favorable. A special form of bucket must be designed to suit such kind of traffic. Simple iron hooks for sacks will, in many cases, be available, and these hooks can be so contrived that on being struck they will drop the sack.

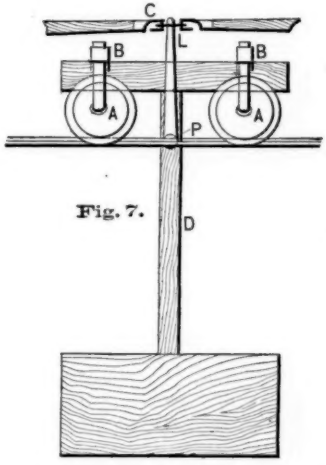


Fig. 7.

The first type of locomotive which was tried on a large scale is shown in Figs. 8 and 9. The motor lies horizontally across the line, and is connected by a form of frictional gearing, which I term right-angle nest gearing, with the edge of a bicycle wheel, W. The shaft of the bicycle has on it two discs, B B, one of which is fixed on the shaft, while the other can slide longitudinally on the shaft. These two discs are pressed together by a spring, D. Their edges bear on the horizontal gripping rollers, A and A, which seize the line. These rollers are supported in such a way as to be free to come together under the pressure of the spring transmitted by the discs, B and B. By tightening the spring, any

required grip can be obtained with no injurious friction, either on the cross shaft or on the spindles of the rollers. This grip is a form of right-angle nest gearing. The weight of the locomotive was taken by wheels, C C, fore and aft. The following defects were observed:—The frictional surfaces both in the upper and lower nests, were too small, and the materials too soft, so that rapid wearing resulted with a consequent increase of friction. Moreover, the grip was so powerful, that the rollers, A A, were capable of supporting the weight, and thus a small inclination of their vertical axis was enough to cause the locomotive to rise, and even run off the line; moreover, the vertical curvature in the rope, or at the posts, required the rollers, A A, to be deep, thus limiting the extent to which rocking was admissible; moreover, very broad pulleys, fore and aft, would be required even for moderate horizontal curves. Nevertheless, this locomotive ran sufficiently well on the one-inch line during an exhibition to the shareholders last autumn. The weight for a five-eighth line of a somewhat improved form of this type, to exert one-half horse-power, on the average, is 200 lbs., an extra half-hundredweight would give one horse-power. The driving wheels, A A, of this example are 6½ in. diameter. The motor makes 9.23 revolutions for one of the driving wheels. One mile per hour corresponds to 473 revolutions per minute of the motor. 35.21 inch-pounds at the motor spindle are required for a pull 100 lbs. at the rail.

Figs. 10 and 11 show a locomotive designed by Mr. A. C. Jameson, when I was personally unable to attend to work.

Fig. 8.

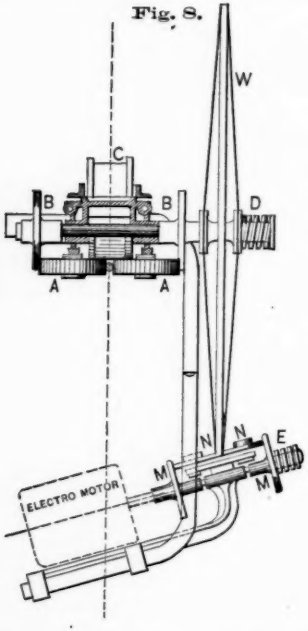
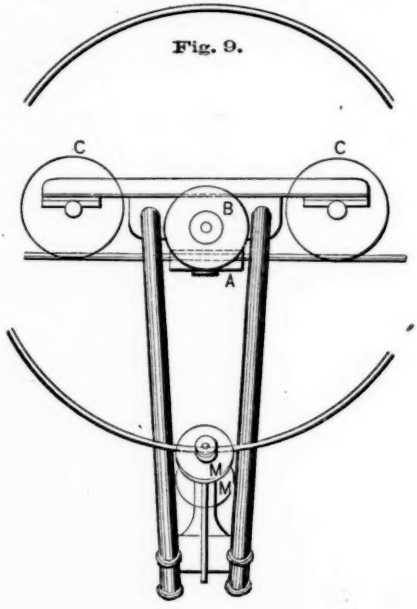


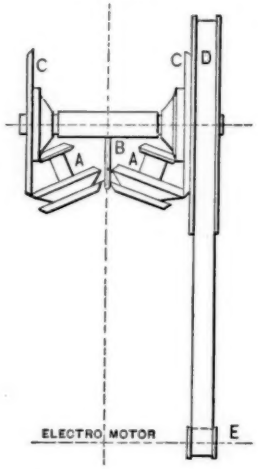
Fig. 9.



This locomotive, which is called the belt locomotive, shows a great advance on its predecessor. The general arrangement of the upper nest grip is retained, but a most ingenious modification has been introduced by which the discs, C C, run on one path on the rollers, A A., while the rod runs on another. In this way, the dirt from the line is never conveyed to the driving disc surface between A and C. Moreover, these frictional surfaces, which are points in the first form, have become lines in the second. This head answers admirably. The weight is car-

ried by a roller, B, between the gripping discs, an arrangement contained in one of my first small models, and wrongly rejected in the first large locomotive. With this subdivision of weight, the gripping wheels are much less likely to rise, and can be made very shallow. In the actual locomotive, these gripping wheels are of an open inverted ∧ shape, which has certainly run very well, although I prefer at present the upright V shape, which closes under the rail, as used in the model before you. Both of the gripping rollers drive as in the first type. The cross shaft is driven by a belt on a 20-in. pulley, D; the other end of the belt runs on a 2-in. pulley, E, on the motor spindle. The friction due

Fig. 10.



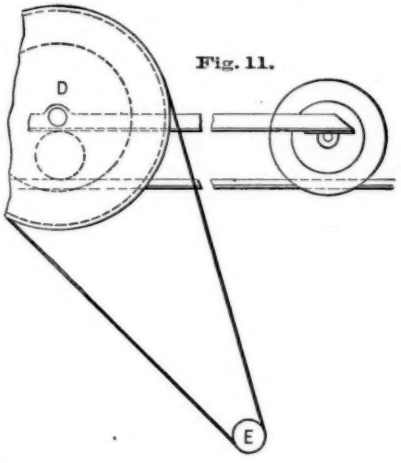
to the pull of this belt on the motor spindle is relieved by friction rollers. This locomotive runs extremely safely and steadily on the line; indeed I am not aware that it has ever been thrown off.

The following are particulars of its construction:—Weights with 96 lbs. one horse-power motor 269 lbs; wheel-base 2 ft. 6 in.; diameters of driving rollers 6 in.; 4.94 revolutions of motor per one revolution of driving wheel. A couple of 60.6-inch lbs. on motor is required for 100 lbs. pull at rail. 276 revolutions of motor correspond to one mile per hour on the rail.

The only improvements I have to suggest in this design are:—1st, the addition of gear which will give a higher speed of motor for the normal speed of four miles per hour, which we contemplate; 2d, the addition of a swivel or bogie arm, such as is used in the model before you; 3d, improvements in the belt connection. Moreover, the machine requires strengthening in some places. It will, however, be seen that none of these points touch the essential features of the design, which might at once be adopted in practice. Worked with motors of the Gramme type, the additional gear would not be required.

Before the belt locomotive had been completed, it was necessary to design a locomotive for the South American line,

Fig. 11.



which I have several times mentioned. I had, meanwhile, constructed the model which is now before you; and this little locomotive, in which the power is transmitted, by ordinary spur wheels, ran so extremely well, that I adopted the general arrangement for the next example on a large scale. This arrangement is shown in Figs. 12 and 13, the grip (C C and B B) is a third variety of the right-angle nest, simpler than that in the belt locomotive. In this form, also, we have line contacts, and two paths for the discs and rod. Where it is desired to drive from both sides, this arrangement is less powerful than that in in the belt locomotive. In the South American locomotive, I drive from one side only, leaving the

off side roller free to revolve as it pleases; this avoids grinding at rapid curves, and the adhesion given by one wheel will be ample in a dry country, such as that where this locomotive is to work. The arrangement of the gearing, E and F, is obvious; it allows the locomotive to lie fore and aft instead of across the line, and this design has some advantages in the adjustment of the weights. The surfaces of the gripping wheels are arranged like an upright V, so as to hold on under the line. This makes it very difficult for the wheels to leave the line, both because of their absolute hold and because the inclination of

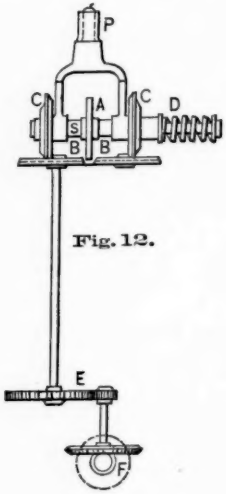


Fig. 12.

the V is such as to favor the action of gravitation in overcoming the friction of the grip, instead of opposing it as in the inverted Λ.

Another feature of this machine is the arm pivoted at P, and carrying the leading wheel, which is again pivoted at M in the arm, as in the case of the trucks. This construction allows the locomotive to traverse curves of 6 ft. radius—a very remarkable result.

The full-sized locomotive has only just been completed, and run on three spans at Messrs. Easton and Anderson's. So far as I am able to judge from the trial, it is likely to be a complete success. It will be immediately shipped for its destination, so that its performance cannot be more fully tested in this country. The following particulars will show that it is much more powerful than the belt locomotive, but it is considerably heavier:—Wheel-base, 2 ft. 6 in.; weight, about 3 cwt. 14 lbs.; 15 revolutions of motor per revolution of driving wheels; diameter of driving wheels, 10 in.; 33.3 in. lbs, per 100 lbs. pull at rail; 504 revolutions of motor per minute for one mile per hour.

I am in doubt at this moment whether to adopt the belt locomotive or the spur wheel locomotive for the next example; it is simply a question of cost, weight, and durability. Either will do the work.

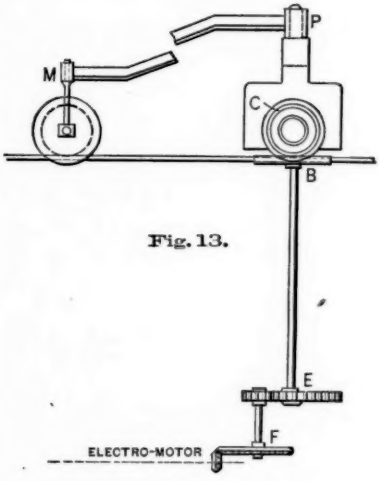


Fig. 13.

In all the arrangements it is essential that the second bearing wheel at M (Fig. 13) should lead, not follow the drivers in regular work. The reverse arrangement lets the rope lead on at an angle with the plane of the roller, causing an injurious grinding action.

Details of couplings have been well worked out, but space fails for their description.

As general features of the train running on the line, I may mention that the deflection of the rod within reasonable limits has very small influence on the resistance. When the deflection on a 50 ft. span was about 2.4 ft., the resistance for a train of trucks, weighing in all 1,260 lbs., was 22 lbs.; and no sensible difference could be detected when the

deflection was materially reduced. This resistance was measured by pulling a train along, span after span, by one end of a rope passing over a pulley on the leading truck, and having a weight hanging vertically from the other end of the rope; the weight thus limited the pull. This pull differs extremely little as the train moves along, for when one part of the train is descending the curve the other part is ascending. It should be noted that during this experiment no special care had been taken to oil the bearings, and I have no doubt this pull can be materially reduced.

I have ventured to dwell at some length on the mechanical problems involved in this form of telpherage, because the experiments made so far have chiefly borne on questions of mechanics. The makers of dynamos can put at our disposal apparatus which will generate day after day, with perfect certainty and regularity, currents of electricity such as will transmit the horse-power generated by powerful steam-engines. These makers have already solved the chief electrical problems which present themselves in connection with telpher lines. They can give us at will constant current or constant electromotive force, high or low, as we may chose. They are now able to arrange their apparatus so that any number of incandescent lamps may be turned off or on, without disturbing the regularity with which other lamps are supplied, and by the same arrangement we are enabled to start or stop any number of telpher trains without disturbing the running of others. The electrical problems of the telpher line and those of electric lighting run in absolutely parallel lines.

The electric motor, although it may be termed a mere inversion of the dynamo, has not as yet been brought to equal perfection, but month by month improved designs, proportions, and materials are being introduced, and the result already attained is sufficient for our purpose. It is all the more encouraging to feel that these results will certainly be surpassed, and far surpassed in the immediate future.

The following short summary of the problem of the transmission of power by means of electricity, may interest those who have not studied the subject. There are three steps in this transmission—1st, we convert mechanical power into electricity by means of a dynamo; in doing so we incur a loss of from 10 to 20 per cent.; 2d, this electricity, in flowing along a conductor, generates heat, representing a further loss, analogous to that resulting from friction in mechanical gearing. This loss, depending on the distance of transmission, the size of the conductor, and the electromotive force employed, is easily computed. 3d, we re-convert the electricity into mechanical power by means of an inverted dynamo, which we term an electric motor. With motors in which large weights of iron and copper are employed, the loss in re-conversion need not exceed 20 per cent., but with light motors, weighing from 70 lbs. to 100 lbs. per horse-power, such as we must employ in the locomotives, I could not undertake with certainty at this moment to effect the re-conversion without a waste of one-half. The effect of all these sources of loss is, that at the stationary engine I must exert about 3 horse-power for every single horse-power which is employed usefully on the line. I look forward confidently to the time when 2 horse-power at the engine will be sufficient to give 1 horse-power to the motor.

To put these conclusions in a more scientific form, I may assume the efficiency of my dynamo as 80 per cent., that of my small motor as 50 per cent. The waste by heat expressed as horse-power is equal to $\frac{C^2R}{745}$ where C is current in amperes, and R the resistance in ohms. The horse-power represented by the current is equal to $\frac{E.C}{745}$ where E is the electromotive force in volts, and C the current in amperes. It follows from the last expression that I may increase the horse-power in three ways, by increasing either E or C, or both. If I increase E, leaving C the same, I do not increase the loss during transmission along the line, no matter what horse-power the given line may transmit. A practical limit is set to the application of this law by the difficulty met with in dealing with electromotive forces above 2,000 volts. Marcel Deprez, taking advantage of this law—first pointed out by Sir William Thomson

—has transmitted seven or eight horse-power over seven or eight miles, through an ordinary telegraph wire, and he obtained a useful duty of 63 per cent., taking into account all the three sources of loss which I have enumerated. With small motors I cannot yet promise a result so good as this, and I merely mention it to let you understand that, in speaking of 3 horse-power for one at the locomotive, I am leaving a very ample margin.

Quitting generalities, I will give some details as to the electrical and other conditions necessary, in two examples, for what may be considered as typical telpher lines:—

First Line.—Length, five miles. Length of circuit, out and in, ten miles. Twenty five trains running at once, spaced one-fifth of a mile apart; speed, four miles per hour. Let each require 1 horse-power on average; let the motor take on the average two ampères of electric current; let the electromotive force near the stationary engine be 840 volts; the electromotive force at the end of five miles will be about 746 volts. The total current entering the line will be fifty ampères at the near end of the line. Fifty ampères and 840 volts represent 56.5 horse-power; of this, 6.5 horse-power will be wasted in heating the line; the remaining 50 horse-power will do work in the motors equivalent to 25 horse-power. In order to give this current of fifty ampères with 840 volts, the stationary engine will require to exert $\frac{1}{8}^{0} \times 56.5$ horse-power, or roughly, 70 indicated horse-power, or somewhat less than three times the useful horse-power. Let us now examine the economical results to be obtained from such a line as this. Mr. Dowson, in an interesting comparison between the cost of horse-power obtained from coal and gas, reckoned the cost per horse-power for a 100 horse-power engine, at the rate of £3 6s. 9d. per annum, to include wages, coal, oil and depreciation. Mr. Dowson would naturally be led to put the cost of steam power obtained from coal rather high than low. I will, however, adopt a very much higher figure, and assume that the power may cost as much as £6 10s. per horse power per annum; this gives £455 as the cost of the 70 horse-power required for my telpher line.

Let the 25 trains each convey a useful load of 15 hundredweight. In a day of 8 hours the line will have conveyed a traffic which we may express as 600 ton-miles—*i. e.*, it will be equivalent to 600 tons conveyed 1 mile, or 60 tons on each line conveyed from end to end daily. If we count 300 working days in the year, the sum of £455 gives £1 10s. 4d. per diem, and the 600th part of this is about 0.604 of a penny, as the cost of the power required to carry a ton one mile.

In Great Britain we ought easily to be able to reduce this below a halfpenny per ton per mile, which proves that the apparent great waste, even of two-thirds of the power in transmission, does not involve prohibitory expense. In calculating the whole cost of transport, we must further take into consideration the cost of the installation. Taking the spans at 70 ft., I estimate this cost as follows:—

Line £500 per mile.....	£2,500
Engine, boiler, and shed, at £20 per indicated H. P.....	1,400
Dynamo and fittings.....	1,000
Twenty-five trains.....	2,500
Contingencies	600

Allowing 12½ per cent. for interest and depreciation, this represents an annual cost of £1,000. Allowing £100 as the salary of an electrician or young engineer, and adding £455 the cost of the power, this gives a total annual expenditure of £1,555 for a daily duty of 600 ton-miles. If we continue to assume the year as containing 300 working days, the total cost of conveying one ton one mile will be found equal to 2.07d. If goods are to be transmitted for long distances, the same calculation applies. We should simply have stations ten miles apart, working lines five miles long on each side of them. This, then, is the practical outcome of the general principles stated at the beginning of this paper. We may expect with great confidence that it will pay investors to convey goods for any distance at the rate of 2d. per ton per mile, by the agency of the suspended telpher line.

Matters are somewhat modified when the traffic is smaller. Making similar calculations for a line one mile long instead of five, with only four trains running at once, we might employ an electromotive force as low as 100 volts; the loss by heating would be insignificant; we should require about 12 horse-power; the work done in

8 hours would be 96 ton-miles. I estimate the cost of installation at £1,600, and the annual cost of working £344, without the annual salary of an electrician. This corresponds to 2.875d., or less than 3d. per ton per mile. One very important feature in respect to the cost of telpher lines is the fact that the larger part of that cost is due to plant, such as locomotives, trains, and dynamos. This plant can be increased in proportion to the work required; thus there is a very moderate increase of cost in the rate per ton per mile for a small traffic as compared with a large one, and, on the other hand, a line laid down for a small traffic will accommodate a much larger traffic with no fresh outlay on the line itself.

There are numerous minor electrical problems involved, but time does not permit me to enter into the consideration of these to-night. It will be sufficient for electricians when I say that I see my way to governing, blocking, and breaking the trains, without ever interrupting the current used to work the motor, except between the line and rolling wheels. We already know that the interruption at this point, although accompanied by a spark, does no injury whatever. I have often been asked whether the frequent reversals involved in the cross-over system do not tend either to injure the dynamo or the motor. I made special experiments on this very point lately with a compound-wound Crompton dynamo and Mr. Reckenzaun's motor with thirty-six coils. I was unable at the commutator of the motor to detect the smallest change in the motion due to the most rapid reversal. At the dynamo commutator I could just see when the reversal occurred, but there was no change of a character to cause the smallest alarm. At the same time I may state that, when from any cause reversals may be thought undesirable, we are in possession of apparatus which we call "step-overs," which, without diminishing the simplicity of the permanent way, enable us to send a continuous and unreversed current. These and similar electrical questions, such as the performance of Messrs. Ayrton and Perry's excellent motors, might possibly have had greater interest for electricians than some of the mechanical details discussed to-night; but I have felt that the main point to establish, in bringing this invention before the public, is that we have in telpher lines a means of conveying goods in an economical manner, by lines, locomotives, trucks, dynamos, and motors, which have undergone their preliminary trials with success, and can be at once applied to the more searching test of performing work for the public. If I have established this fact, I think you will have no difficulty in believing that the subsidiary electrical problems have been, or will be, readily solved. I hope that at a future period these will be brought before you in detail on many occasions by many men.

In conclusion I will enumerate some of the uses to which telpher lines may be put. They will convey goods, such as grain, coals, and all kinds of minerals, gravel, sand, meat, fish, salt, manure, fruit, vegetable; in fact, all goods which can be divided conveniently into parcels of two or three hundredweight. If it were necessary, I should feel no hesitation in designing lines to carry weights of 5 or 6 cwt. in each truck. The lines will carry even larger weights, when these, like planks or poles, can be carried by suspension from several coupled trucks. The lines admit of steep inclines; they also admit of very sharp curves. Mere way leaves are required for their establishment, since they do not interfere with the agricultural use of the ground. They could be established instead of piers, leading out to sea, where they would load and unload ships. With special designs, they could even take goods from the hold of a ship and deliver them into any floor of a warehouse miles away. When established in countries where no road exists, the line could bring up its own materials, as a railway does. Moreover, wherever these lines are established, they will be so many sources of power, which can be tapped at any point, for the execution of work by the wayside. Circular saws, or agricultural implements, could be driven by wires connected with the line, and this without stopping the traffic on the line itself. In fine, while I do not believe that the suspended telpher lines will ever compete successfully with railways, where the traffic is sufficient to pay a dividend on a large capital, I do believe that telpher lines will find a very extended use as feeders to railways in old countries, and as the cheapest mode of transport in new countries. In presenting this view to you, I

rest my argument mainly on the cost of different modes of transport, which may, I believe, be stated approximately, as follsws:—

Railway, 1d. per ton per mile; cartage, 1s. per ton per mile; telpher lines, 2d. per ton per mile; and let it be remembered that, in taking the cost of cartage at 1s. per mile, the first cost and maintenance of the road is left wholly out of account; whereas, in my calculations for the telpher line, allowance has been made both for establishment and maintenance.

HARBORS AND DOCKS.*

By E. SHERMAN GOULD, C. E.

Reviewed for VAN NOSTRAND'S MAGAZINE.

Of books of the class to which the volume before us belongs, it may be commonly predicted, off-hand, that the latest is the best. Harbor and dock engineering is so largely built upon observation, is so much an affair of "trial and error," and, as a science, is as yet in such an early period of its development, that that work which, from its late date, can give the most complete record of success and failure in the various operations which it describes, must, in so far, be the most valuable one.

In the present volume Mr. Vernon-Harcourt gives a very thorough and comprehensive review of the subjects enumerated on his title page. His method of treatment is purely descriptive, and rather general, in its avoidance of detail. This was unavoidable if the whole of the wide scope undertaken was to be brought within the limits of 650 pages, and probably the work possesses a wider usefulness from this fact. It is certain that the guiding principles involved are brought into a clearer prominence by not dwelling at length upon merely accessory facts or details of construction.

In the opening chapters the fundamental data of marine engineering are presented, by a statement of the facts concerning the winds, waves and tides. The wind and the tide are the great causes of those violent wave agitations, which it is one of the objects of this branch of engineering to encounter and neutralize. Without the wind there would be no waves; indeed, when we witness the awful onset of the ocean, dealing its staggering blows against the free end of a breakwater, we but behold the physical embodiment of the invisible breath of the tempest, which, were its destructive energy not stored up in the waves, would howl harmlessly around the pier-head. From the chapter which treats of the winds, we transcribe the following valuable and simple formula, verified, it seems, by observation, which gives the pressure of the wind in pounds on the square foot, expressed in terms of the velocity in miles per hour, thus:

$$\frac{V^2}{100}=P.$$

Passing to the subject of waves and wave motion, we find in Chapter II. these matters treated from the point of view of their more striking generalities, and without the detail into which Monsieur Comoy enters in his "*Etude pratique sur les Marées fluviales*" (see VAN NOSTRAND'S MAGAZINE, September, 1884), but in a way which, to the general reader at least, will perhaps be more acceptable than a full mathematical investigation. We notice here a tendency to question Mr. Russell's division of waves into those of oscillation and translation. Mr. Harcourt says: "It is evident, however, that if great wind waves are waves of translation, the same must be true of the smaller wind waves, though in a less degree. As soon as it is admitted that wind may produce some translating effect upon water, the distinction between waves of translation and oscillation cannot be maintained."

As an instance of the marvelous power of the sea, Mr. Harcourt mentions the destruction of the free end of Wick

* Harbors and Docks; their Physical Features, History, Construction, Equipment and Maintenance. By Leveson Francis Vernon-Harcourt, M.A. Two vols. 8vo, text and plans. Oxford, 1885.

breakwater during a storm in December, 1872. A mass of cement masonry of 1,350 tons, about 45 feet wide, 21 feet high, and about 26 feet long, was on this occasion gradually turned around upon its base, and finally tilted bodily off its foundation. After reading this account we are relieved to learn that the Wick breakwater is exceptionally exposed, owing to a combination of tidal and other influences, the waves rising occasionally to the height of 42 feet, although the depth at low water is only 30 feet.

We would call particular attention to the long and very able Chapter III., in which tides, currents, and their effects are admirably treated upon. Indeed, we should say that these subjects alone, of the theoretical ones touched upon in his book, are examined by the author with some considerable degree of close investigation.

These preliminaries being cleared away, the author enters upon a classification of the various types of harbors, their forms and characteristics. This part of the work seems well done, and to be well done must have cost much thought and study, as those will admit who have ever attempted to reduce a great number of facts and instances to a systematic order. When well done, such classification is of itself an immense aid to the study of any subject.

Next comes, in natural sequence, the consideration of the structures destined to improve or create the harbors thus described.

The principles and classification of these structures being established, practical applications are made by citing an exhaustive list of existing harbors—a sort of *catalogue raisonée* of some of the principal seaports of the world, in which the different harbor improvements mentioned have been practiced, with their results.

Rather than attempt even the briefest review of the whole of this most interesting and instructive portion of Mr. Harcourt's book, it will serve a better purpose to dwell for a moment upon some one or two leading types of harbor improvements. We will select the comparison afforded by the two systems of jetties for the improvement of backwater harbors—that is, harbors largely kept open by the scour of the ebb tide, the waters of which had previously entered on the flood. These two systems are, that of parallel and that of converging jetties.

Parallel jetties were the first to suggest themselves for the improvement of tidal harbors by deepening their bars, when a growing commerce caused larger and larger vessels to seek their docks. To a considerable extent the desired effect was accomplished, but, says Mr. Harcourt, "These jetties, however, when projecting from a shore along which there existed a natural drift of sand or shingle, acted like groynes, and arresting the travel of material along the coast, produced an advance of the foreshore, which eventually compromised the depth at the extremity of the jetty channel. This difficulty was met by a periodical extension of the jetties, till at last, when the drift was considerable, the jetties extended a long distance in front of the port, as may be seen at the present day at Dunkirk."

One among the objections to parallel jetties is, that they impede the free entrance of the flood tide into the harbor, by forcing it to travel through a long and narrow channel, and thus diminish the amount of backwater available for ebb-tide scour. To avoid this the upper portions of the jetties have been built of open timber work, with partial success only, because not only was a large portion of the tide water still excluded, but also from the jetties not being tight, they allowed a considerable dispersion of the ebb tide, which, to be fully effective, should be confined to the channel it is wished to deepen. Sluicing basins have also been in some cases resorted to, but of course all such appliances are not only costly, but introduce an element of complication which it is always very desirable to avoid. On the other hand, solid converging jetties "provide a reservoir of tidal water close to the entrance, afford ample room for the reception of vessels or the working of dredgers, reduce the entering waves, also in some cases by being kept low, allow the littoral current to pass over unchecked, and in every case, owing to their sloping course, present less obstacle to the regular flow of the current around the entrance."

Among other differences of the two systems is the width of opening. "In converging jetty harbors, a greater width

of entrance is generally given than in the case of the parallel jetty harbors. These harbors are of more recent origin, and consequently designed for a larger class of vessels; they also commonly extend into deeper water, and are frequently more exposed. In these cases the width has to be a compromise between conflicting conditions. The object of these jetties is to secure a deep entrance; if the entrance is made very wide, a bar is liable to form from the inefficiency of tht scouring current; if the entrance is very narrow it induces a rapid current, which, indeed, maintains the depth, but is prejudicial to the entrance of vessels, and a narrow passage is naturally objected to by seamen. . . . . Charleston, the finest jetty harbor in the world, is designed to have an entrance 2,000 feet wide."

It is certainly gratifying to an American engineer to record the above tribute to one of the boldest projects ever undertaken in harbor improvements.

One of the most interesting descriptions is that of the creation of a fine harbor at Cherbourg, one of the largest marine works ever undertaken, and which, commenced in 1783, was only completed in 1853. There being no natural harbor of large dimensions suitable for a naval station on the southern coast of the English channel, the French engineers undertook to make one, by simply throwing an immense detached breakwater across the mouth of Cherbourg Bay, leaving an entrance east and west. This, after a long contest with many difficulties, was finally and triumphantly accomplished.

The chapters upon docks are perhaps less interesting to American engineers, from the fact that the small rise and fall of the tide in our harbors renders the use of docks, properly so-called, unnecessary. We extract the following remarks upon the use of concrete in dock-building, as they have a wider interest than this special application: "The experience at Chatham shows that a good concrete wall can be constructed with as small a proportion of cement as 1 in 12; but unless very careful supervision is exercised in the mixing of the concrete, it would be safer to adopt a somewhat larger proportion of cement. . . . . The economy effected by the use of a concrete wall is occasioned not merely by the cheapness of the materials, but also by the facility of construction and the saving of plant."

We also quote the following: "The quay walls at New York, whilst built upon a similar principle to those of Rouen, have a foundation of rubble stone in addition, as the piles in this instance do not reach solid ground, and therefore they have to be consolidated, and the base enlarged by a mound of stones. It is satisfactory to find that a stable quay wall can be built with only a moderate amount of settlement on a bed of silt; the success of the operation being due partly to the adherence of the piles, partly to the consolidation of the silt around the piles by the small cobble-stones, and partly to the broad base of the larger rubble mound."

The author naturally finds the quays of New York deficient in appliances, such as traveling cranes, hydraulic power, and the like, which are always found on the great European docks. He mentions the use of compressed air in the working of dock cranes, but while admitting that it is superior in some special cases, as at Portsmouth, considers that hydraulic power is generally the best.

The remarks made upon dredging seem somewhat meager and scattering. We are rather surprised to find, in so complete and general a *compte rendu*, no mention made of the excellent results obtained by General Gillman in dredging, while under way, with the centrifugal pump at Charleston, Savannah, and elsewhere. We remark also that the author seems to hold the scaphandre, or diving suit, in low estimation, as compared with the diving bell (of which he mentions an improved variety) for submarine work. In his observations upon lighthouses and beacons we fail to find any mention of the advantage of range beacons in entering or leaving ports.

A work based to so great an extent upon facts and observation, leaves but little opening for criticism, its merit or demerit depending almost entirely upon the intelligent classification and presentation of the collected material. In these respects we have nothing but praise to bestow upon Mr. Harcourt's volume. We find in it a vast store of well-selected facts and instances of marine engineering,

carefully ordered and arranged. Every type of harbor and of harbor improvement finds a place and abundant illustration from existing works. The large mass of material necessarily involved, is so condensed and stripped of superfluous matter, that what would seem at first sight to be a superficial review, is found upon examination to contain all the central facts. The accompanying atlas of plates is exceedingly complete; in fact everything mentioned in the text finds its graphic representation in the atlas. Were we disposed to find any objection to the plans of the different harbors, it would be that there was hardly enough of the territory back of the actual sea front given, but such an addition, while it would undoubtedly add to the value of the illustrations, would greatly increase the size of the volume. All the sections of piers, breakwaters, etc., are very good and complete.

THE SEWAGE-DEPOSITING TANKS OF FRANKFORT-ON-THE-MAIN.

By W. H. LINDLEY, M. Inst. C.E.

From Abstracts of the Institution of Civil Engineers.

THESE works are now in hand, and will be completed in the early part of 1886. The sewerage-works, comprising a network of about 160,000 meters of sewers, have been in execution since the year 1867, and are for the greater part complete. The subsidence-tanks form the completion of the outfall section of these works, and by the plan adopted two outfalls were needed, one on the right, the other on the left bank of the river, discharging into the river main at some distance below the town. While the more costly outfalls were in progress, a temporary outlet was made use of at the gasworks, and this now serves as a storm-overflow. At first the admission of the water-closets was left an open question, as the sewers were constructed to attain a number of important results which could not be delayed till this question, then so largely debated on the Continent, was settled. Professor Pettenkofer, at that time opposed to the water-closet system, however, when called in, reported in this case in its favor, and the prohibition was withdrawn. Owing to the rapid discharging power of the town sewers, the excrementitious matters and paper came down in considerable quantities in a very unchanged condition into the river, and occasioned nuisance, and steps had to be devised to screen out the grosser suspended matters from the sewage water.

After many difficulties, caused by the landed proprietors, and after much pressure had been brought to bear upon the municipal authorities, plans for dealing with the sewage water were prepared so far back as 1876–77, but for various reasons the matter remained in abeyance. In fact, though constant negotiations were in progress, it was not until October, 1882, that the scheme was finally settled. It was decided to bring all the sewage-water to the left bank for treatment, and to effect this the sewage is carried in two wrought-iron pipes, each 0.75 meter (2.46 feet) in diameter, laid as an inverted siphon under the bed of the river. Each of these pipes can deliver 500 liters (110 gallons) of sewage per second. The extra pipe is added to provide for subsequent increase of population, as the one pipe will suffice for the whole of the present flow, including a rainfall equivalent to the sewage in volume. For heavy rainfall, which might exceed the carrying-capacity of the siphon, a storm overflow is provided. The main outfall sewer is capable of discharging 2,700 liters (294.2 gallons) per second, but it was considered unnecessary to arrange for bringing all this volume across the river, as it would not have been feasible to construct tanks for so large a volume of rain-water, and it would have simply been turned into the river as storm-water. For the normal volume of sewage, the one pipe only is employed, the area of cross-section normally in use is thus reduced to one-half what it would have been if one pipe of equivalent carrying power to the

two laid had been adopted; the normal velocities have thus been about doubled, and this will, it is expected, be sufficient to scour out the siphon. Plans are given, showing the arrangement of the tanks and works. The process to be employed will comprise a system of mechanical purification, aided by chemical purification. It is thought expedient to remove first from the sewage such substances as can be separated by simply mechanical appliances. These impurities consist of floating bodies, coarser suspended matters, and the heavier substances which sink to the bottom. The precipitants to be subsequently employed are sulphate of alumina, and lime. As is well known, the acid in the former substance combines with the alkaline matters in the sewage, and sets free the alumina, as a bulky flocculent precipitate, which, by its affinity for organic substances, entangles them and carries them down. As sufficient alkalies are not present in the sewage to liberate the whole of the alumina, a certain proportion of lime has to be added. The advantages of this treatment are set forth. The plan of tanks adopted will not be carried out at present in its entirety, but will provide for future extensions. The whole scheme comprises twelve parallel tanks, of which only four are now in progress of execution.

The chief point to be considered in respect to the construction of the covered tanks was whether they should be above ground or sunk, *i. e.* high-level or low-level. The former plan would involve continuous pumping to a height of from 5 to 6 meters. By adopting the alternative arrangement of low-level reservoirs, it is possible, without any pumping, to pass the sewage into them by gravitation, and to discharge the clarified effluent into the river on an average of three hundred days in the year. Only on forty-seven days in each year does the river rise to such a level as to involve partial pumping of the contents; and only on about one day in three years is the river so much in flood as to render the use of storm overflows necessary for the whole of the sewage. The extra cost of sunk tanks would be compensated for by the avoidance of pumping. The machinery for pumping and treating the sludge, which is the part of the process presenting the most difficulty, has been concentrated at the lower end of the tanks to which the sludge naturally gravitates. The dimension and inclination of the main outfall-sewers, the arrangement of the sand-intercepting chambers, the by-passes, overflows and storm-outlets, the mode of admitting the water into the tanks, of passing off the clarified effluent, draining the sludge of a maximum amount of its water, and finally drawing off the sludge, are fully described. The four tanks are each 82.4 meters (270.3 feet) long $\times$ 6.0 meters (19.68 feet) broad. The bottom is formed of an inverted brick arch, and falls 1 meter in the direction of its length; the depth of the water in the tanks being 2 meters at the inlet and 3 meters at the outlet end. Each tank has a normal capacity of 1,100 cubic meters (242,106.7 gallons), and is calculated to receive from 4,000 to 5,000 cubic meters daily, under average conditions of flow. The contents are thus about 25 per cent. of the whole quantity to be treated; the corresponding proportions at the following English towns, by which the Author was guided, being—Leeds, 22½ per cent.; Coventry, 30 per cent.; Aylesbury, 26 per cent.; and Burnley, 16 per cent. The total average daily volume of sewage water to be treated at the present time amounts to 18,000 cubic meters (about 3,961,746 gallons). The average velocity of the sewage-water in passing through the tanks is 4 millimeters per second (about 0.786 foot per minute). It is intended to empty each tank weekly. Full particulars are given concerning the machinery for pumping and preparing the precipitants, the stores (which will contain 200 tons of sulphate of alumina and 180 tons of lime), as also the proposed plan of dealing with the sludge and detritus, and the general method of carrying on the works.

THE 110-TON GUN.—The English government have ordered three 110-ton guns, and of these one is to be delivered in October next, another in January, and the third in April, 1886. The price per gun is £19,500; the weight of the projectile is 1,800 lbs.; the charge is 900 lbs. of cocoa powder; the muzzle velocity is 2,020 ft. per second; the maximum powder pressure is 17 tons per square inch. The velocity and pressure are, of course, only estimated, although they are based on the experience gained with the Italian guns.

EXPLORATION—AND THE BEST OUTFIT FOR SUCH WORK.

By Major-General the Hon. W. FIELDING.

From the "Journal of the Society of Arts."

I think it best to preface this paper with the Latin expression, *Quot homines tot sententiæ*, which may be very freely translated—a tot of men afford a quart measure of opinions. But, seriously speaking, it would be quite absurd for any one individual at any one period of the world's existence to attempt to lay down the law as to how explorations should be carried out.

The most that I can attempt to do is to speak in general terms on the whole subject, using such knowledge as I have gained during my own travels in various quarters of this globe. In order to treat the subject as exhaustively as the limit of time at our disposal will admit, it will be well to divide it under two headings.

1. On exploration generally, and the manner in which the subject should be considered.

2. On the outfits recommended for use by explorers under varying circumstances.

The first heading we must again subdivide into—(*a.*) Scientific explorations. (*b.*) Commercial and geographical. (*c.*) Military. (*d.*) Explorations arising purely out of a love of adventure.

Now, scientific explorations differ or vary exceedingly in their intention and their scope. Their scope depends again upon their intention, and their duration depends upon both these. For instance, botanical explorations may have for their aim a new genus, a new species, or a new variety only of some species. The scope of such exploration may embrace one or more islands in the Pacific Ocean, or the whole of the interior of some unexplored continent such as New Guinea. The duration must depend upon:—(1.) The means of transport to the primary base of operations. (2.) The means of locomotion over the whole or the various portions of the country to be explored. (3.) The physical difficulties to be encountered from man, and from natural obstacles. (4.) The financial means available in this conflict with the difficulties, foreseen and unforeseen, of exploration.

There is much truth in the old saw "money makes the mare to go," and with plenty of money many of the difficulties of exploration are greatly lessened; yet I would here impress on you that plenty of money may be a source of serious trouble, and of much worry to the unexperienced explorer. He is tempted to buy everything he is likely to want, and so encumbers himself with an amount of baggage which he finds it impossible to transport from his base, and from which he finds it most difficult to make a selection.

It would be useless to mention in detail to a general audience the various instruments, appliances, and chemicals, which should be taken by the explorers in search of botanical, horticultural, geological, mineral, or zoological specimens. Specialists have each their individual special outfit, suitable for the purposes they have in view.

There are, however, certain articles of outfit which are necessary to every explorer of uncivilized, of partially or totally unexplored countries, their quality and quantity must vary with the nature, scope, and duration of the work to be done.

Most of the researches enumerated above necessitate either slow progress through a country, or a lengthened stay in various selected districts best suited for the operations of the specimen hunters. An explorer, bent on commercial or geographical discoveries, naturally contemplates travel over long distances, and, generally speaking, with less physical and fewer natural obstacles to be overcome in proportion to the distance to be traversed. On the other hand, however, he generally has to travel, and indeed to live, in a continual state of preparation for defence.

The military explorer must again work on different lines. His business is to seek information in countries occupied by a hostile population, with whom, however, his nation is not necessarily at war. He must travel unostentatiously, almost

alone, and must avoid all hostile contact with the inhabitants. Such were Colonel Burnaby, when he went to Khiva, and Captain Gill, during his explorations along the Persian frontier, and his subsequent travels in the interior of Northern China. The explorations for purely sporting purposes, or arising from an innate love of adventure, require consideration equally careful, and knowledge seldom acquired otherwise than by personal experience.

For our purpose it will be sufficient for us to divide our inquiries into two different channels. To facilitate and to narrow the question, we will decide that the exploration is to be partly geographical, and so far scientific that the explorer has to report in general terms on the geological and mineral resources of the country to be traversed. There are no roads, but little timber, and that sparsely scattered, except near water, of which the quantity is small, and the quality always questionable and often bad. The rivers in drought do not exist except as chains of muddy ponds, whilst in flood they become impassable for weeks, and overflow their banks often to the extent of from three to fifteen miles on either side. In such a country game would be scarce, and could not be depended upon as the only source of animal food to the explorers. The above data are sufficiently explicit and sufficiently difficult to meet almost every case.

We must now come to consider the manner in which an exploration of such a nature is to be carried out.

1. Would it be possible to establish some one or more subsidiary bases of operations. If the reply be in the affirmative, then comes the questions—(*a*) Where shall they be? (*b*) What shall be stored there? (*c*) How shall these be conveyed thither?

Now the answers to these questions must depend upon the nature of the exploration *i. e.*, if the intention be to return to the place of starting, or to traverse a continent from sea to sea.

2. What is the nature of the transport to be? If wagons, are they to be light (though strong), many in number, and of different sizes, or are they to be few in number, heavy and solid in construction? How are they to be drawn—by oxen, by horses, or by mules? If wheeled transport be out of the question, what are the pack animals to be, camels, horses, or mules, or some of each of these animals?

Each and every one of these questions has to be carefully considered, because on the solution of one question so many others must depend. It may be well here to enumerate some of the chief circumstances which tend to govern the choice of transport.

1. Nature of the soil generally. If the country to be traversed be very broken in character, covered with thick forests, and known to be traversed by sluggish streams with deep slimy banks and bottoms, it is clear that wheeled transport, unless of a very special character, would not be suitable. Neither would such a country be practicable for camel transport; and yet there can be no doubt but that more stores can be easily carried on wheels, and by camels, than any other way by land. There are, however, very few countries in which exploration with wheeled transport may not be carried out, provided time be no object, and plenty of patience and perseverance be available. This brings us to the consideration of the general outfit of an exploring party.

1. As to stores.

2. As to the mode of transporting them.

3. As to the construction of the wagons, the pack-saddles, harness, &c.

4. As to the mode of packing them and storing them.

1. As to the stores. These must be sub-divided under the headings of (*a*), provisions for the mouth; (*b*), materials for obtaining food, or for offence and defence; (*c*), materials for facilitating the locomotion.

In the choice of provisions, care must be taken to select such articles as are wholesome, nourishing, small in bulk, and not liable to deteriorate by keeping. There must also be variety, so as to promote health, and a proper proportion of such articles of consumption as would diminish the risk of scurvy.

Of meat the best sort is preserved beef in tins. There is very little to choose between that preserved in Australia and that preserved in America, north and south. The tins should not be too large, and they should be rectangular and not cylindrical in shape. Essence of Beef

(Brand's or Liebig's, in tins or in skins) is a most valuable form of meat. Flour and oatmeal should be packed in block tin boxes, of various sizes, containing from 1 lb. to 4 lbs. each. Sugar should be cane sugar, powdered, and packed in ½ lb. rectangular canisters. Tea—the best for the purpose is Goundry's compressed tea; it is manufactured in tablets of ¼ lb. in weight, and subdivided like chocolate tablets, into eight portions, one of which is ample for tea for three or four people. Being wrapped in lead paper, it stands any climate, and I have known it to keep good for five years. Salt should be kept in stone or thick glass jars, with screw or cork-lined stoppers. Lard should always be taken, and should be kept in stone jars, capable of being rendered air-tight. There is an excellent form of compressed and dried vegetable tablet manufactured in France; and there is also a preparation of dried potato, in powder. No expedition should be without these to keep off scurvy, that terrible scourge and bugbear of all explorers. Ginger, peppers, red and black, should be carried in thick glass pickle bottles, with air-tight glass stoppers, edged with cork. Brandy for medicinal purposes should be carried in small wooden kegs, covered with thick felt, and with a locked covering to their bungs. A provision of lime juice should be similarly carried in kegs of different sizes. There should be several spare kegs of the same description, kept constantly filled with fresh water. In addition to this provision of water, each animal should have a canvas water-bag slung by a strap round his neck. These bags keep the water cool, and each should have the neck of an old soda water bottle sewn into the orifice used for filling it; the vessel can thus be easily used, without detaching it from the animal carrying it. Water-bags on the same principle, only much larger, are made of well-seasoned leather, and are slung by straps and iron rings on to a pack-saddle. At first the water has a nasty flavor; but the bags soon cease to affect the taste of the water, and are indispensable on long waterless marches in a hot climate.

Whilst on the subject of water, it may, perhaps, not be out of place to impress upon you the necessity in observing the greatest care in the selection, and, generally speaking, the after treatment of water. As a general rule, the only water which can be drunk with safety, without fear of evil consequences, is that which springs directly out of the ground, from rocks, or which is obtained from a permanent running stream, the bed of which is not muddy, and on the banks of which there is not an exuberant vegetation. Even in the case of water issuing from rocks, care must be taken to avoid water issuing from copper or lead-bearing rocks. In these cases a small quantity of sulphuric acid would at once detect the presence of the mineral in dangerous quantities, as the water would become discolored.

In most countries subject to drought, the water requires special treatment; mechanical filtration is seldom practicable, or even safe. I have come across it as thick as pea soup, and sometimes covered with a growth of green or red weeds. In such cases, the first operation is that of skimming with a skimmer made out of a forked stick, with a pocket handkerchief or other piece of linen stretched tightly between the forks. This done, scatter a pinch of powdered alum into the vessel in which you have collected the skimmed water; this will cause a great deal of the matter in suspension to precipitate. Then pour the water slowly into a filter filled with the charcoal of your last night's camp fire, mixed with any sand or fine gravel which may be obtainable, and which you have previously washed. It must then be boiled, and skimmed whilst simmering, and only when no more scum arises on the water is it really fit or safe to use. It is a good plan always to fill the kettle—or, still better, the cooking pot—with water the last thing at night, and put it at the edge of the camp fire to simmer (not to boil), and always to fill up the water kegs and bottles from what is left over from each morning's cooking. It is also a good rule never to drink plain cold water in the tropics. Each man should carry in his pocket half a handful of oatmeal, and put a pinch into his pannikin of water when he fills it for drinking.

I once traveled 1,400 miles across a portion of the center of Australia, and began my journey after a drought which had then lasted eighteen months, and which only broke up the day I reached

the sea coast. It was only by the strict enforcement of these precautions that (under Providence) I never had a case of illness from fever or from dysentery. Personally, I always carried in my pocket a few "thirst lozenges," which are, I believe, nothing except a compressed form of Lamplough's Pyretic Saline.

Before closing the enumeration of the *provisions de bouche*, it is well to add a list of the medicines and surgical instruments necessary to every expedition:—Rhubarb, essence of ginger; about 100 pills of colocynth and henbane; about double the quantity of quinine pills, made up in small doses of three grains each; some opium pills; a couple of bottles of Dover's powders; four bottles of sweet spirits of nitre; about 100 pills of podophyllin in small doses; camphor, and chlorodyne. Two lancets, two abscess knives, two catheters, two enemas; some surgical needles, and some silver wire thread for sewing wounds; a silver probe, and two vein or artery forceps; a syringe with various nozzles for various uses. Sticking-plaster of various sorts, and some prepared lint and medicated wool; and some vaseline, carbolic acid, and carbolic soap. All the medicines should be in glass-stoppered bottles, the stoppers having been lubricated with pure glycerine previous to insertion. The medicines, &c., should be divided into at least three portions, so that each wagon, or each detached party, should have a complete set of everything. There is no greater mistake than to have everything in one medicine chest. All boxes should be avoided, as in a very damp or a very dry climate boxes are apt to come to pieces with the rough handling that every package gets at the hands of those who often have to do the packing and unpacking of animals two or three times each day.

Clothing.—Take as little as possible when starting from England, as you can get most articles necessary for explorers at the place from which the wagons would make their start. Of personal attire, the following are those which I consider sufficient for most expeditions:—Four shirts made of grey flannel, with two buttons on each wristband, to admit of them being worn loose or tight. Four long merino drawers, double seated and double down the inside of the thighs. Four pair of thick knitted woolen long stockings. Two cholera belts, one of knitted worsted, the other of flannel about a quarter of a yard wide and three yards in length, to be wound round the body or fastened with a safety brooch. Six silk pocket-handkerchiefs (white), and of the thickest and best quality. They are useful sometimes when traveling in the very early morning, to serve as a curtain against the sun's rays, which often at that hour strike with great force on the nape or side of the neck under the hat. A Norfolk jacket of good woolen serge or light tweed, made double-breasted, so as to be worn either open with the lappels buttoned back, or buttoned across double over the chest and stomach. It should be made like a garment known by miners as a jumper, not cut in at the waist, but merely kept in at the waist by a belt. This belt should be made of two pieces of soft leather, about 2½ inches wide, and stitched together at the edges so as to admit of dollars or other coins being kept in the belt and slipped in at either end, and prevented from falling out by a flap and button at each end.

If a sword has to be carried, it is best carried fastened on to the side of the cantle of the saddle by a round strap and button of leather. If a revolver has to be worn, it is best carried in a frog supported by a webbing belt over the right shoulder, which should be kept in its place by the waistbelt.

The best hats are of gray felt, of a helmet shape, with means for ventilation round the edge and at the top. They should be provided with a chin strap, to be worn when riding fast or against a strong wind. The best boots are those known as the Paliser boot. They reach nearly to the knee, and are laced up for about six inches from below the instep, so that the boot can be always easily got on and off, whilst remaining watertight. I prefer those made of porpoise hide to any other, as they are lighter and more supple in wear.

Dogskin driving gloves should always be taken, as their use prevents sun boils, blisters, and many sores arising from thorns, &c., on a journey. Breeches should be made very loose, except just below the knee, where they should be fastened with a buckle and strap, or tied with thongs of porpoise hide. A

hunting whip with a hammer handle and a long brown leather lash is always useful, and is a necessity where there are many spare horses to be driven along with the party.

Camp Necessaries.—India-rubber buckets, two to each wagon, should always be carried, to be used for watering the horses whilst in harness. Palkee hammocks, made of waterproofed canvas, are the best and most portable form of bedstead, and it is always unadvisable to sleep on the level of the ground. The blankets should be loosely sewn together round three sides so as to form bags. This plan saves many a sleepless night. Moreover, it keeps snakes from getting in between the blankets. A waterproof sheet, with eyes round the edges, is most useful, as when thrown over the ridge pole of the hammock it can be lashed to the sides of the hammock, and serve as a complete shelter even in the heaviest storms of rain and wind. A light folding chair, or if this be too large, a beach seat with a back, is a great luxury, and is almost a necessity in wet ground.

We have now to consider the selection of such materials as are necessary to secure supplies of fresh provisions to protect life. First and foremost are guns. These should be breech-loaders of the simplest possible construction, and of 12 bore. Each gun should be provided with 20 steel cartridges. These are really indestructible, and are very easily reloaded and recapped; and having a female screw turned for a distance of an inch inside the cartridge, there is no difficulty in making the wads to keep in position.

For ammunition, shot of all sizes should be taken, the larger slugs for use against man or large animals. Powder should be carried in two small copper magazines, each containing about 7 lbs. of powder in half-pound canisters, fitting into the outer cylindrical copper case. These canisters should have screw tops with leather washers to them. The canisters should always be kept full so long as there is any powder in them. When a canister cannot be filled with powder, it should be filled up with cotton wool, rags, or even crumpled up soft paper. It must be remembered that any expedition is liable to be reduced to pack animals only, and then the attrition is so great that everything which can rub, soon gets rubbed to the finest dust.

When it is known that there are rivers or lakes, it is well worth while to take a casting net, and even a small Seine net of strong tanned twine. A large provision of hooks and fishing lines of all sizes should always be taken, as they are not only useful in the obtaining of a change of diet, but are very valuable as an article of barter with natives.

For personal defence the best weapon is the largest sized Colt's revolver, with a stock which can be used at the shoulder, and is detachable. When on horseback it is best carried in a bucket, like our cavalry carry their carbines. A good one shoots with wonderful accuracy up to 100 yards. A hunting knife, of a pattern of my own, I have found the best, as it is light, and yet strong enough to cut away a fairly large branch. The sheath is of bamboo, and there is room in it for a knife and fork of steel, flat, with wooden handles screwed on each side of the shaft. The blade of the hunting knife is made light by having two deep grooves cut out of the thickness near the center of the blade, so that, whilst the blade is made lighter, it is also thereby rendered much stiffer.

The only other stores which we have to review are those required in reference to locomotion—*i. e.*, spare harness, leather, rivets, and copper wire for repairs, spare nuts, screws, iron clips, splinter bar caps; tools, such as augurs, center bits and braces, saws, files, chisels, screw wrenches, screw-drivers, gimlets, awls, sewing needles, wax and strong thread, felt for saddle cloths, roll of flannel for saddle linings, raw green hide, and skins of dried leather, half-inch iron rods, flat iron hooping for strengthening splicings, adzes, jack planes, spoke shaves, sharpening hones, files, punches, rasps, horse shoes, nails, and shoeing tools, felling and trimming axes, crosscut saws, hand saws of three or four different sizes, from 3 ft. 6 in., to 15 in., clamps, light and heavy hammers, a few pairs of blacksmith's pincers and tongs, a couple of good bellows (hand), and an assortment of nails, screws, copper and steel, D's, buckles of different sizes, and straps of various lengths and widths.

Having enumerated the stores necessary to an expedition, the next thing to

be settled is the means of their transport.

It is rare that the only transport available is man, but yet in some tropical countries, covered with thick jungle, and where the ground is too rocky and broken even for mules, donkeys, or cattle, it is necessary to use men, and men only, for this purpose. Chinese and Japanese coolies will carry great weights balanced on two ends of a long bamboo cane, which rests on the shoulders. Sometimes two men will carry a heavy package for weeks at a stretch, slung on a bamboo cane between them. In Africa and South America, the natives prefer to carry heavy things on the top of the head. When packages are carried slung, the slings should be made of plaited ropes of green hide, kept well greased. Everything else wears out almost immediately. Every expeditionary force should be provided with pack-saddles, and with the means of constructing them. Personally I prefer the sort in use by the Basque population in the Pyrenees. It has the advantage of simplicity and cheapness of construction, and of being easy to use and to repair. The best form of camel pack-saddle is the one used by the Arabs, who contract with pilgrims to and from Mecca. Numnahs of felt should always be used, both with saddles and pack-saddles. If carefully adjusted, they admit of animals being kept in work with sore backs, should it be necessary.

The best form of bridle for all animals are those made entirely of tanned twine webbing. It is quite impossible to break them, and they are comfortable both to the heads of the animals and to the hands of the rider or driver. Besides this, they do not become slippery in wet weather, neither do they require any care to keep them in working order, as leather does in hot or dry climates.

Hitherto, we have treated entirely of man or of animal transport; but there are very many countries where it is not only possible, but very advisable, to adopt wheeled transport.

The class of wheeled transport must depend upon three conditions.—1. The nature of the country to be traversed (I put roads as out of the question). 2. The quantity of stores to be carried. 3. The quantity and quality of the animals available for its traction.

It is now almost an exploded idea that a wagon must of necessity be a heavy, cumbersome vehicle, with thickness and weight as the governing virtues of all its component parts. The Boers and others in South Africa still adhere to the old pattern, from habit and ignorance more than for any known reason. In America and in Australia, where the country is just as difficult to traverse, very much lighter vehicles are used with great success.

It is well to have several sizes and types of vehicles in every expeditionary outfit. Two-wheeled carts, long and broad, with draught from the shafts and outriggers at the sides of the shaft, which would admit of its being drawn, if necessary, by three horses abreast; four-wheeled wagons, light and medium, with pole draught, with side springs, and india-rubber buffers on the axles, these latter being connected by a perch. All wagons and carts should have lever brakes, capable of being worked by hand and foot by the driver. There should also be iron skids, or shoes and chains to be used if required, in addition to, or to replace the brake. The chief things to be borne in mind in the construction of vehicles for expeditionary transport are—

1. Great simplicity of construction.
2. As few parts as possible.
3. Screw clips should be used in preference to bolts and nuts, inasmuch as every bolt weakens the wood traversed by it, in proportion to the diameter of the bolt.
4. All parts should be made of such shapes that they can be readily copied and replaced by an unskilled workman.
5. The wood should be perfectly seasoned, neither so dry as to diminish its toughness, nor too full of natural moisture or sap, and no iron should be used except where absolutely incapable of being broken, or where the use of wood would be incompatible with strength and endurance. It may, however, be used where, in the event of its breaking, it could be easily replaced by wood.
6. The height of the axles from the ground should be the same, and not less than two feet. It is seldom that a wagon has to be turned at a very acute angle, therefore no great amount of "lock" is necessary. When making a track through a forest, much time and

labor are saved by cutting the trees off at about two feet from the ground, as they are not nearly so large in girth at that height, and it is less fatiguing to the men felling them with cross-cut saws or axes.

7. The various parts of each wagon, excepting the bodies, should be interchangeable, so that in the event of a complete breakdown, the unbroken portions of the disabled wagon could be utilized in the repairs of others. This is very essential, as tending greatly to the reduction in the quantity of spare stores.

We may, therefore, proceed to consider the construction of a wagon under the following heads:—

1. The under carriage, including the wheels.
2. The mode of traction.
3. The body (including the tilt where necessary).
4. The means for suspension of the body.

As stated under paragraphs 6 and 7 above, the parts should be interchangeable, and the axles should not be less than 2 ft. from the ground. It follows, therefore, that the wheels should be of the same diameter, and not less than 4 ft. 2 in.

One of the great troubles in all explorations, especially in very hot and dry climates, is the difficulty experienced in keeping the wheels in working order. The spokes shrink, and unless this is immediately found out and remedied, by calking the gaps left in the wheel stock and felloes with white lead and cotton waste, or with oakum, wet gets in, and the end of the spoke soon rots away. The slightest sign of looseness anywhere in the wheel must be at once attended to and remedied; green hide (cut in strips half an inch wide) wound in and out the spokes near the stock, greatly strengthens a wheel of which the parts have begun to shrink. In some very dry climates, no wheel of ordinary construction will stand. On one of my expeditions I had each night to take off all the wheels and lay them in water till daylight, in order to keep them together, and even with this precaution, the wheels eventually turned inside out and fell to pieces. There is, however, a form of wheel which seems to me to be likely to last longer than the sort in ordinary use. It is that known as the Madras pattern, and the invention is claimed by an American named Sarven. The spokes fit round an iron stock, and are kept in position by two circular plates, bolted from outside to inside the wheel. This would admit of a broken or damaged spoke being easily replaced, or they could be wedged up from the center by the insertion of thin bits of iron, tin, or some hard substance, between the inner end of the spoke and the stock. Another difficulty arises from the difficulty of keeping the tires on. In England, it is easy enough to remedy the tendency which all tires have to lengthen. They can be cut, shortened, and reshrunk on the wheel. In exploring work, the tires, as a rule, do not permanently increase in circumference, as they do from use on hard roads here, but the wheels shrink away from them with the heat, and this same heat expands the iron tire, and so causes it to lose its contact with the felloes.

The evil results arising from these causes may be minimized in two ways:—

1. By constructing the tires slightly convex on the inner circumference, and by making a corresponding concavity in the outer circumference of the felloes.

2. Sometimes, however, the shrinkage is so great, that it becomes necessary to cut and shorten the tire. As it is almost impossible to secure a good weld to reclose it, it has to have the two ends filed to a feather edge, brought together, and then firmly clipped to the felloe at either end of the splice. The tire may be wedged tight, and secured with clips.

The axle-trees should be of the best toughened iron. bedded in tough timber, and clipped. In length they should be 3 ft. 8 in. to 3 ft. 10 in.

The greater the breadth (in moderation) the greater the stability of the vehicle when moving across an incline. Moreover, with all the wheels of equal diameter, the lock is increased by leaving a greater space between the wagon body and the front wheels. The axle-trees of the fore wheels and hind wheels should be connected by a straight perch made of tough wood, such as hickory. Inasmuch as there is very little "lock" required, there is no necessity for any complicated or delicate wheel-plate (or fifth-wheel). A stout transom, with an iron

eye through which the king bolt would pass, and the axle-bed connected with the splinter bar by two wheel-irons, and braced at the rear by a stout sway bar, is all that would be required. These should be all straight pieces as far as practicable, and clipped together (not bolted). In very broken and precipitous ground the pole might be taken out, and the movement controlled by ropes held by men.

As regards traction, it would be a great gain if the pole could be dispensed with, inasmuch as in very rough ground it knocks the wheelers about sadly, and it is more frequently broken when working in difficult ground with untrained horses and bad drivers than any other part of a wagon. It is, however, sometimes necessary, and must therefore be provided for. It should be attached firmly to the splinter bar, and the bar allowed to move freely. The attachment should be by means of two iron bars passing through eyes clamped on to the splinter bar at the two ends, ending in two iron stays coming out from the bars at an angle of about 20 degrees, and clamped on to the pole. The pole is thus worked freely up and down, and the pressure would be taken off the jaws of the futchells by the two jointed iron bar stays.

The hanging pole, moreover, necessitates a contrivance to relieve the horses from having constantly to support its weight. This can be done by having a strong hook, fastened by a clamp, at about one-sixth of the length of the pole, from the splinter bar. On to this hook is fastened a chain, or strap of plaited raw hide, which, running through a sheave (firmly fastened by a broad plate bolted on to the footboard), is hooked at its other end on to a hook fastened to the under side of the front of the body of the wagon. These hooks must be strong, and have a broad bearing where fastened to the carriage body. It would even be advisable to introduce a spiral spring at one end of the chain, to take off the sudden strain occasioned during the passage over very rough ground.

When driving four or more half-trained horses on broken ground, it is safer to use no pole piece and bars, but to use long traces made of green hide rope, kept up by loops hanging from the wheeler's trace carriers, the leader's traces being kept apart by means of very light hickory bars, kept from slipping by green hide thongs passing through the ends of the bars, and fastened through loops in the leader's traces. It is well, however, to be able to use the pole and bar draught; with that view, the pole piece should be fastened by clamps, counter sunk round the pole head. The hook should be made on a twist, to avoid the necessity of using a strap, as with wild horses it is necessary to be able to detach the leaders with as little delay as possible.

Germane to the subject of traction is the question of how to bring it into control. The ordinary skid or shoe cannot be depended upon in rough, rocky ground, as the wheel is apt to jump out of the shoe. The ordinary hand brake, acting on the front of the hind wheels, is insufficient. To these two should be added a friction brake working on the hinder circumference of the hind wheels by means of a bar, shod at the two ends, which can be compressed against the wheels by a screw working on the end of the perch, prolonged for this purpose.

If the tires should be secured by clips at any part of the journey, the projections would interfere with the brake blocks, so the brake blocks should then be applied with enough pressure to prevent the wheels revolving.

Suspension.—If a very rough country has to be traversed, it is well to have the body of the wagon suspended on springs, so as to save the damage done to the stores, as well as to the wagon by the jolting.

The best form of spring appears to me to be that adopted by some of the best carriage makers in the construction of gentlemen's omnibuses for station work with heavy loads.

The springs are single, and coupled to the scroll iron on the body by a shackle, inside which is an iron coupling or robin. These are practically unbreakable, as the coupling takes off the strain from any sudden and heavy jolt. There should be, however, india-rubber buffers fastened on to the body to minimise the shock, if it were to be so severely jolted as to come down suddenly on the bed of the spring. I have found it very useful to have a strong swinging tray (made of strong ash planks one inch thick) fastened so as

to hang between the axle-trees. The planks should not be too close together to prevent axes, spades, picks, and such like heavy articles being attached to the planks by means of thongs tied round the planks, and passed through holes in the handles of the implements. The whole tray should be constructed that it can be readily taken to pieces, and the planks utilized in the passage of boggy grounds, or in the sandy beds of rivers, or in running the wagons up very steep inclines in soft ground. I have found them of great use, especially in deep ground, where they can be put under the wheels. Another advantage arising out of the use of this tray is, that as in it are placed heavy articles, the center of gravity is brought lower than if the same weights were carried in the wagon itself. There should also be a small water barrel, covered with felt, hanging under the wagon at the rear.

Covering.—Every explorer's wagon should have a tilt, to serve as a shelter from sun and rain. It should be made of waterproofed canvas, and have a fall-down piece in front to shelter the driver, and a curtain behind, with thongs to enable it to be kept closed when needed. The framework is best made of hickory, fitting into rectangular sockets well outside the framework of the body, so as to allow of ventilation from under the sides, and to give greater head-room space in the interior. There should also be a ridge pole of hickory running through rings clamped on to each rib. This ridge pole can be utilized for slinging a hammock in case of sickness or wounds during the march.

Fittings.—Under the driver's seat should be a movable box, in which to place all the tools and materials necessary for mending the harness, or any part of the wagon. The box should be constructed in trays, so that each thing may have its place, and be readily available. Each wagon should have its camp kettle, which should be slung on hooks under the rear of the body. On the splash-board there should be hung a stout leather bag, in which might be kept strong twine, a sharp knife in a sheath, and a hatchet and hand-axe for ready use. Each wagon should have a strong lantern for use, with good wax candles.

Harness.—The great desideratum is to have as little harness as possible, and that it be strong without being heavy. Headstalls and bridles may be made of stout webbing dipped in tan. The reins should be round, and of plaited green hide. There should be as few buckles as possible, and the ends in the driver's hands should never be buckled, but merely kept together by a loosely made reef knot, which can easily be undone in the event of its being necessary to let the leaders go clear. The traces should either be made of plaited raw (or green) hide, or of the best two-inch rope.

It is well to be provided with both collar and breast draught, so as to be able to change from one to the other form of draught in case of need. Copper rivets and copper wire are most useful for mending harness and saddlery, and plenty of it should be with the stores. There should also be plenty of hobbles to prevent horses from straying too far from camp at night in search of feed. Some horses, however, become so clever in hobbles that they can even gallop in them. In such places the best plan is to attach a cord from the head collar to the hobble of one leg. It is well to have a few cattle bells to attach to the necks of some of the horses most likely to stray. By these means much annoyance and delay in starting are to a great extent avoided.

Horseshoes.—Although in most expeditions the horses are not shod, it is wise to take a small supply of shoes and nails, to be used in the event of it being necessary to cross a tract of stony or rocky country, where horses would soon wear down their feet, and become tender-footed and useless. The class of shoe must depend upon the breed and class of horses used. The Arabs, who ride their horses over very rocky and stony ground, most frequently shoe their horses with plate shoes, covering the whole of the sole; but this form is not suitable to a wet soil or a stiff clayey country. Every party should have a blacksmith amongst its members, and it is well that most of the party should be able to shoe a horse without driving the nails into the quick.

Packing the Wagons or Pack Animals.—There are certain principles in packing, whether it be wagons or pack

nimals, which should never be lost sight of.

1. To make each wagon or group of pack animals complete in itself, *i. e.*, it should contain everything necessary to the existence of those in charge.

2. So to arrange the stores that those most frequently used should be so packed that nothing else need be disarranged in order to get them out.

3. To arrange the stores in such a manner that the heavy packages should be equally distributed over the surface of the wagon, or amongst the beasts of burden, and that the lighter articles should always be so well secured as to prevent the possibility of their becoming loose, and thus spoiling their contents.

I have known hard biscuits reduced to powder by the omission to pack the case with paper, so as to keep the box always full; clothes worn into holes by attrition from their having been placed in contact with hard corners; maps, and even books, destroyed in the same manner.

Now, as regards the packing of animals, it is quite impossible to do more than lay down first principles, viz.:—

1. That the panels of the pack saddle must be well and evenly padded; this should be looked to at every halt and promptly remedied, otherwise sore backs will ensue.

2. That the weights should be quite evenly divided on either side of the saddle, so as to avoid the necessity of having to draw the girths too tightly, or of having to stop frequently to re-arrange and trim the burdens.

3. The weights should be kept low, so as to lower the center of gravity as much as possible. This is especially necessary when any mountainous country has to be traversed.

4. The packages ought not to stick out too much laterally, especially when wooded country or a narrow rocky pass has to be traversed.

5. Where practicable, it is best to put some soft or yielding package outside the others, as the pack animals often run against one another, and damage in such cases might arise both to the animals and to the packages, if the latter were hard and unyielding. Moreover such a plan enables the surcingles to be better arranged.

6. Never attempt to pack an animal alone. The weights having been arranged on the ground, the animal should be led between them, and the packages should be placed on the hooks simultaneously.

7. The same precautions should invariably be taken when unpacking, as at that time it is so very easy to "wring" and to "rick" an animal's back.

8. At every halt of more than an hour the packs and pack saddle should be removed, and, where practicable, the backs should be washed with salt and water, alum and water, or carbolic soap and water, then rubbed dry; and just before repacking, the back should be brushed with a penetrating bush, to remove all grit, sand, or dander, as almost all horses, mules, and asses, roll on the ground as soon as their saddles have been removed.

9. After each day's march, the back of every animal should be examined, and the slightest tenderness or shrinking observed. The smallest sore or abrasion should be carefully washed with carbolic soap, and dressed with vaseline ointment. If there be no spare pack horses, and rest be an impossibility, then a numnah of thick felt should be interpolated between the back and the saddle, and a hollow, or even a hole, cut in the numnah, to prevent any pressure coming over the sore place.

The same treatment should be observed with respect to the shoulders and withers of the harness animals, remembering the old proverb, "a stitch in time saves nine."

There should be an intelligent, capable man in charge of all the wheeled transport, another in charge of the pack animals, and a man in charge of the spare and sick horses. Each driver should be responsible for his wagon and team, and there should always be a mounted man with the wagons, and with each detached wagon. There should be a cook in whose charge all the stores should be, and he should ride, if possible, so as to go forward with the advanced party, to light the fires, collect the wood, and, where necessary, improve the water supply. He should carry a hatchet and a small spade.

The man charged with the supervision of the sick and spare horses should have another man with him, as it is often

necessary, especially at the commencement of a journey, to leave a man behind to search for and bring up horses which have strayed, and sometimes even gone back from the camping ground.

In every exploration where wheeled transport is employed, there should always be a reserve of a class of horses called "emergency" horses, *i. e.*, horses able and willing to give a steady and strong pull. They should be well bred, strong horses, and should not be used except when required in heavy ground.

In countries where the water supply is uncertain, it is the best plan to send two men ahead with a spare horse, to explore for water. When found, one of the men returns on the spare horse.

As a rule, from ten to fifteen miles may be considered an average march in a new country, in which there are no physical difficulties. I have, however, more than once, only been able to progress two miles in the day; whilst in order to reach water I once had to make, in three successive days, marches of forty-one, forty, and forty-three miles. These were, however, made with pack animals, and without wagons. Before concluding this paper, it may be interesting to most of you to hear a few remarks on the manner in which exploration for water is generally conducted. Experience, and even common sense, tells us that in a hot or a dry climate, animals and birds are but very seldom found far from water towards sunset, and that at sunrise they generally leave the vicinity of water on their search after food. Observations as to the direction of the flight of birds, and especially of all the parrot tribe and the carrion birds, will generally lead to the discovery of water.

In almost every country there are some descriptions of shrubs and trees which will not grow except in the vicinity of water; and even where this water may not be obtainable on the surface, it can, under such conditions, be found by sinking in suitable spots in the beds of the streams where those shrubs or trees are found. I once traveled for three days down the bed of a river which was quite dry, and yet by sinking from six to ten feet in the bed, a sufficient supply was obtained. It seems to be a provision of nature that in very hot and dry countries the streams almost invariably run for considerable distances under ground. With a very rudimentary knowledge of geology, and by the observance of the natural signs of water peculiar to each country, travelers may, and do, often find water where an unobservant man might die of thirst. This all-important question is of more interest than usual at the present time, when an expeditionary force, composed of European troops, is about to undertake the opening up of the trade route from Suakim to Berber, on which the last two stages, fifty-three and fifty-two miles respectively, are without any visible supply of water. Personally, I have but little doubt, from the geological formation of the country, and from the conditions of the water supply along the rest of the route, that these two dry stages will be bridged over by the discovery of a subterranean supply of good water Let us hope that capable men may be employed in the exploration of that part of the route, and that our expeditionary forces on the Nile and the Red Sea may be able to join hands at Berber, and thence proceed to re-establish the prestige of British arms in the Soudan.

THE MANUFACTURE OF "COCOA" POWDER.—The War Department have adopted the new brown gunpowder known as the "cocoa" powder, and it is now being manufactured at the Government Factory, Waltham Abbey, on the principle introduced from the United Rhenish Westphalian Powder Mills, Cologne. Some trials of this powder have taken place at the proof grounds in the government marshes, Woolwich, under the direction of Major Hemans, Royal Artillery, the proof officer, and in the presence of Mr. E. Kraftmeier, the representative of the Westphalian works. Two thousand cases of this description of powder have recently been purchased, and samples have been fired in an 11-inch breech-loading gun. Ten rounds were fired to test the pressures and velocities, the charge of powder being 295 lbs., and the weight of the projectile 655 lbs. The pressures were taken at five positions within the gun, and the velocity, as usual, at the muzzle. The mean pressure was 16.5 tons per square inch, which is two tons lower than stipulated, and the whole of the velocities were between 2,002 feet and 2,010 feet per second. The pressures were also exceedingly regular, the highest being 17.6 tons, and the lowest 16.3 tons, while the mean variation in velocity was less than 2 feet. The brown powder creates a very thin smoke, and the committee at the School of Gunnery, Shoeburyness, have reported that it does not obscure the targets.

THE RESILIENCE OF STEEL.

By WILFRED LEWIS.

Proceedings of the Engineers' Club of Philadelphia.

The problem of storing energy in a convenient shape for transportation or domestic use, suggests a wide range of possibilities, and opens up a large field for the exercise of inventive genius.

A good solution of the problem is urgently needed and as earnestly sought, but, as yet, all efforts seem to have failed to accomplish practical and economical results.

That continued labors in this direction will finally be rewarded by success, is certainly within the bounds of possibility, but meanwhile it appears as though everything that will not work must be tried first, and that a vast amount of knowledge must still be gained by sad experience and disappointment. The subject of this paper was suggested by a case in point, the proposition being to utilize the energy stored in a number of steel springs for the propulsion of street cars.

The question which naturally arose, was: how much energy can be stored in a given weight of steel?

The answer to this question was at first sought from the data given in standard works of reference, but these were found to be so meager and indefinite, that the writer was led to make some experiments, to be described. Before going into particulars, however, it will be of interest to note some of the various ways in which energy can be stored, and the comparative position of steel among them.

This has been done by a writer in the "English Mechanic and World of Science," for November 2d, 1883, who compares the methods of storing energy by means of steel springs, India rubber, compressed air, hot water, and electricity, and expresses the results in terms of the weight of material required to store one horse-power per hour.

Of steel, he says that fifty tons of watch-springs, all fresh wound up, would not supply one horse-power for one hour.

Five or six tons of India rubber, or about three hundred pounds of compressed air, including the weight of a steel case to contain it, would yield about the same result, and the weight required to store this energy by means of hot water or electricity, is said to be at present about the same as for compressed air, with the future probabilities in favor of the storage battery. He also goes on to show that the energy stored in the shape of horse flesh, will yield 2,000,000 ft. lbs. per hour for five hours. The weight of the horse being taken at 1,500 lbs., gives for the energy stored about 6,670 ft. lbs. per lb. of the animal, and as this is a more convenient form of comparison, we find for the previous examples, that 18 ft. lbs. can be stored in a pound of steel; 100 ft. lbs. in a pound of rubber; 6,600 ft. lbs. in a pound of air, including its steel case, and about the same in a pound of hot water or storage battery.

According to the same writer, the energy stored in the coal, water and boiler of a locomotive, will yield about 25,000 ft. lbs. per pound of all the materials used in storing.

From these statements it appears that steel has comparatively but little capacity as a reservoir of power, although, as is well known, it has long been used successfully and even preferably, for such light work as the running of clocks and toys, where convenience and availability are the main points in view. If the figures just given were accepted as correct, it would seem hardly credible that any one would attempt the task of propelling a street car, upon the basis of 18 ft. lbs. per pound of material used for driving, but upon investigation it was found that considerable work had already been done upon an experimental car, for the purpose of having a practical test.

The invention is described in *The New York Scientific Times and Mercantile Register*, for December 15th, 1883, as a "wonderful system, by which horses on street car lines will be abolished."

The working parts will be of phosphor-

bronze, polished, and there will be 80 steel springs, each 3 inches wide, by $\frac{3}{32}$ inches thick, and 60 feet long, which, it is said, can be wound in two minutes, by a stationary engine at the depot.

This may be very quick work, but what is more to the point. the figures are given to show that by the use of this system, the car companies can carry passengers for three cents, and make as much money as they now do at the present rates.

In the case of the springs running down before reaching the station, a wise provision has been made by the introduction of a "powerful hand-winding arrangement, so that the engineer can apply the arrangement while the car is in motion, and thus reach the station without delay," but, unfortunately, no provision appears to have been made for winding up the engineer.

"The car is also provided with an ice attachment for winter use, a governor to regulate the speed, and about sixteen hand-levers, for various clutches, all conveniently and easily handled."

Upon inquiry it was not surprising to find, that although considerable work had been done upon the polished bronze portion of the car, no experiments had yet been made to determine the duty of the steel springs in question; and having become interested in this part of the subject, as a matter of scientific importance, I undertook the following investigation to determine the possible resilience of steel:

The elasticity of a steel bar may be developed by extension, compression, torsion or flexure, the latter being a combination of the two former.

All steel springs are brought into action by torsion or flexure, not because any more work can be stored by these methods, for in reality there must be less than by either extension or compression, but because the forces involved are more manageable.

In flexure and torsion, the metal is strained in proportion to its distance from the neutral axis of the section, and therefore, the full amount of elasticity is developed only in the extreme fibers, but in direct tension or compression every particle of metal must yield its full share of duty.

It is impossible to determine what this may be under the most favorable conditions, because it may never be known when those conditions are reached. Everything, of course, depends upon the quality of the steel, and its physical treatment. No satisfactory data upon these points could be found; the effect of hardening was said to increase the elastic limit and ultimate strength, and to diminish the elongation and reduction of area, but the effect upon the modulus of elasticity was not given in connection with these other changes.

This modulus is given by Rankine, at 29,000,000 for soft steel, having a tenacity of 90,000 lbs., and a working strength of 30,000 lbs., and at 42,000,000 for hard steel, having a tenacity of 132,000 lbs., and a working strength of 44,000 lbs.

We have also, upon his authority, that within the elastic limit, the modulus for compression is sensibly equal to the modulus for extension.

Prof. Burr, in his work on the "Elasticity and Resistance of the Materials of Engineering," gives a table showing the effect of tempering upon the elastic limit and ultimate resistance, and the highest values there given for the elastic limit are 58,350 lbs. before tempering, and 107,650 lbs. after tempering. The ultimate strength corresponding, is 110,340 lbs. before, and 169,430 lbs. after tempering.

These results were obtained from a mild grade of French steel, and, although this elastic limit is probably above that of average steel, it is of course below what might be expected of higher grades.

Assuming Rankine's figures for hard, untempered steel, we find that its resilience is 7 ft. lbs. per lb., under direct tension. That is to say, any given quantity of untempered steel is just capable of storing enough energy to raise its own weight, through a distance of 7 ft., and that a car without weight driven by such a spring, without any loss through friction, could not quite manage to ascend a hill eight feet high, without the assistance of the engineer.

Supposing the elastic limit to be 107,650, instead of 44,000, as given by Rankine, we have about 43 ft. lbs. per lb., a much better result, but still insignificant in comparison with horseflesh, or compressed air.

According to Rankine's formulæ for spiral steel springs, the torsional resili-

ence of steel is greater for the same intensity of stress than that of direct tension or compression. This is due to the lower modulus of elasticity for torsion, which prevails against the loss from incomplete straining of the fibers. The modulus for shearing is given at 12,000,000, and assuming 44,000 for an elastic limit, we have 12 ft. lbs. per lb. of metal for the torsional resilience of steel springs.

In order to put these calculations to a practical test, I procured two door-springs, of $\frac{3}{16}$ inch wire, and about $3\frac{1}{2}$ feet long. One end was held in a vise, and, upon a lever attached to the free end, weights were suspended until a deflection of 180° was produced. The first specimen, which was $37\frac{1}{2}$ inches long, deflected 90° with a weight of 4 lbs., at 15 inches rad., without taking set, and 180° with 8 lbs., at $11\frac{1}{4}$ rad., showing a set of about 30°. The second specimen gave better results. It was $40\frac{1}{2}$ inches long, between clamps, and deflected 90° with a weight of 4 lbs., at 13 inches rad., and 180° with a weight of 8 lbs., at the same rad., showing not more than 5° set. Taking the latter case, we find the greatest shearing stress to have been 80,000 lbs. per square inch, and the resilience about 43 ft. lbs. per lb. Substituting these results in the general formula for torsional deflection, we find the modules in this case to have been about 10,000,000, which corresponds closely with the values generally given.

Dividing the general formula for torsional resilience

$$R=\frac{\pi}{16}\frac{f^2 d^2 l}{C}$$ by the weight $$W=\frac{.28\pi d^2 l}{4}$$

and reducing to ft. lbs., letting f= intensity of stress, and C= modulus of elasticity for distortion, we have, for the energy developed per lb. of steel,

$$E=\frac{.075 f^2}{C}$$

The ultimate value of E, from these experiments is, therefore, about 48 ft. lbs., allowing 80,000 lbs. for f and 10,000,000 for C. If the spring is strained to but half its elastic limit, but $\frac{1}{4}$ of this amount, or 12 ft. lbs., per lb., can be obtained, the same as deduced from Rankine's formula for the safe load.

In order to test still further the resilience of spring steel, I procured two clock springs wound in spiral forms. The first, which was $\frac{3}{8}$ inches wide, and .014 inches thick, weighed 605 grs., and was consequently 60 inches in length.

It was mounted upon a mandril and tested for each revolution in winding and unwinding, by a weight sliding upon an arm attached to the mandril. By this means the friction of the coils in pressing against each other could be measured, as well as any set which might occur. At twelve revolutions the spring appeared to be wound up, and it then supported a weight of 1 lb., at 3 inches rad., with a variation of $\frac{1}{4}$ inch either way in the radius. The tension was in all cases proportional to the number of turns, and no set was apparent after unwinding.

It was expected in this test that a much lower result would be found than in the test for torsion, both on account of the higher modulus for bending, and also on account of the character of the stress in developing a smaller proportion of the inherent energy in the steel. But the result is surprising, for we have without doubt, 9.42 ft. lbs. in 605 grs., or 108 ft. lbs. per lb., and assuming the neutral axis to be in the middle of the ribbon, there must have been exerted a transverse resistance of 240,000 lbs. per square inch upon the outside fibers, and this too within the elastic limit.

To satisfy myself on this point, and to be sure that this remarkable strength was not due to the combination of superimposed layers, I fastened a short length of the spring between wooden clamps, one of which was fixed so as to use the spring as a cantilever to sustain weights attached to the other. In this way the spring supported a weight of 1 lb. at a radius of 4 inches, when an apparent set took place.

The transverse strength of the steel was thus found to be over 320,000 lbs., with an elastic limit closely approaching that amount.

The other spring, which was $\frac{1}{2}$ inch wide by .022 inches thick, and weighed 2,040 grs., showed even better results. After making twelve revolutions it sustained a weight of 2 lbs., at 6 inches rad., developing 45 ft. lbs. of work, or 154 ft. lbs. per lb., and showing a transverse elastic resistance of 300,000 lbs. per square inch. The energy developed by

any layer of the spring is proportional to the square of its distance from the neutral axis, and we find by integration that the energy developed by flexure is one-third of that which is possible by direct tension or compression.

We should therefore expect to obtain from this steel, under tension or compression, 462 ft. lbs. of energy per lb. of steel, when strained up to 300,000 lbs. per square inch, and this gives for the modulus of flexure the value of 30,000,000.

This low modulus of elasticity, in connection with such a high degree of transverse strength, seems to give new life to the project which at first appeared so hopeless, but it is still doubtful whether any great success can be anticipated under this more favorable light. Assuming 154 ft. lbs. per lb., which is probably far in excess of what can be obtained on a large scale, and allowing ¼ of the total weight for the weight of the springs, and 50 per cent. for the efficiency of the driving mechanism, we have about 20 ft. lbs. of available energy per lb. of load moved. On a level track or down grade this might be sufficient to run a mile, but it would be entirely inadequate to overcome ordinary grades, or to endure many stops, even if a portion of the energy were returned in stopping. The approach to Spring Garden Street Bridge, for instance, would doubtless prove insurmountable.

The experiment, however, will soon be tried, and the result, if unsuccessful, will at least be instructive. It is believed, in conclusion, that the results here given upon the resilience of steel are the highest yet recorded, notwithstanding the fact that they are constantly being realized in practice, and that the demonstration of their truth is within the reach of any one who will take the trouble to make it.

SOME GENERAL CONSIDERATIONS AFFECTING STRUCTURAL DESIGN.

By WM. H. BURR, C. E.

From Selected Papers of the Rensselaer Society of Engineers.

The time is even now not far distant in the past when the chief object in the design of a bridge or similar structure was considered to be the proper determination of the area of cross section of the ties, posts, and upper and lower chord members; and what was twenty-five or thirty years ago a problem of no ordinary complication, has not at the present time yielded in all its parts to the most approved analytical and experimental methods. While a clear line of demarcation has long been drawn between that class of structures which are free from ambiguity in stress determination and those that are not, the compression member as placed in a bridge structure still possesses a resistance at least partially indeterminate, in spite of the accuracy with which it may be treated when placed in end conditions identical with those of the testing machine. It has been clearly established by numerous tests that the best forms of cross section will enable the length of a flat or pin-end strut to vary between wide limits without essentially changing the resistance of the column, but the upper chord of a pony truss still defies exact treatment, and refuses to wholly yield even to gusset plates or knee braces. It can be readily conceived that the centers of resistance of the ends of a column may within certain limits keep pace with the center deflection both in direction and rate of motion, and thus preserve the resistance essentially constant, even with a considerable increase of length; but what can be taken as the effective column length in the pony truss upper chord? It cannot be its distance between panel points symmetrical with the center, for gussets and knee braces do give some steadiness, though not complete rigidity, nor can the lateral stability yielded by the tension web members be completely disregarded.

The demands of extraordinary span

lengths have called into existence new types of truss and stiffened structures requiring novel but successful analytical methods; but the resistance of the swing bridge with variable movement of inertia of truss section and ever-changing thermal effects, remains at the present time, at best, in only an advanced conventional state, in spite of many analytical attacks. It is true that much has been done, but the design of a swing bridge is at present a task which the competent and conscientious engineer must approach with some hesitation.

But if these difficulties bear the marks of age, there are a host of others that have been reached by the rapid advance of modern bridge building. Many of these depend solely upon correct conceptions of true functions of details which originally were either not recognized at all or very incompletely; others have arisen from the adaptation of improved or new materials, while perhaps the most complicated questions relating to structural design are those involving the manner of application of the moving load.

The office of that important detail, the pin, which gives the stamp of individuality to American bridge structures and forms the basis of their superior excellence, has been clearly discovered, though original failure to do so caused the early disrepute in Great Britain of a feature of construction which, under more skillful design in this country has been the foundation of the only system permitting exact analytical determination in the truss. The proper development of the eye-bar head has long since produced a member equally strong in all its parts, and by judiciously designing and arranging, an indefinite number may be grouped on the same pin without unduly increasing the size of the latter.

The importance, and even necessity of applying increments of chord stress directly to the center of the metal intended to carry them has long been recognized and accomplished in all first-class design, except in the matters of upper and lower lateral bracing. The problem of a perfect system of lower lateral bracing for a through bridge is one that has not yet been satisfactorily solved, though a degree of no inconsiderable excellence has been attained. Fortunately, however, the chord stresses of the lateral systems are usually so small in comparison with those of the vertical loads that a very material eccentricity, even in the application of the increments, is not of great importance, and is easily provided against by stiffness of details.

Among the more frequent questions confronting the designer, in consequence of combination of the more elementary shapes in the production of proper forms of strut section, is that of the latticing uniting channels or composite shapes of plates and angles. Although this mode of strut construction has long been in use, a rational method of proportioning the latticing is yet to be determined. Conventional rules of more or less crudeness have been followed, but none of them appear to recognize the true office of this important portion of the column. It certainly does not perform the part of a compression member to the extent of the difference between the resistance of the two halves of the strut in laterally unsupported columns and that of the complete post itself. It impresses upon each half a transverse load which produces a bending in direction opposite to that caused by the longitudinal thrust, and its office is, therefore, simply to hold the two parts in fixed positions relatively to each other. The amount of the tension or compression in a direction normal to the axis of the column exerted by this latticing measures the least allowable longitudinal section (as well as rivet area) of the stay plates at the strut ends; for they must exert an amount of force equal, but opposite in kind, to that of the latticing. The amount of this force and its distribution is as yet unknown, and this part of the strut problem remains unsolved.

It is probable that there is some extravagance of material in the best of present latticing, but it is a judicious practice under circumstances of such deficient knowledge.

These are a few examples only of the many that might be cited, of the difficulties of design that confront the engineer at the present time, and while they do not admit of precise solution, it is absolutely essential that resort be made to some experimental or conventional method which, though not accurate, shall neither be extravagant on the one hand,

nor permit the margin of safety to fall below the proper limit, on the other.

The rapid improvement in processes for the production of structural materials during the past few years has led to the application of the lower grades of steel yielded by the open hearth and Bessemer processes, to the construction of bridges and roofs. The nature of this material and its behavior in certain constructive manipulations is not even yet completely understood, but its superior capacity to wrought iron in many operations of the rolling-mill, its increased ductility and perfect homogeneity, no less than its greater tensile and compression resistance, but chiefly its successful and satisfactory application to a number of existing bridges, renders the question of its ultimate general use merely a matter of a comparatively short time, and gives to all questions connected with it an unusual demand on the attention of the engineer. Although we have as yet only begun to traverse the problem connected with this material, much has already been done not only in solving some preliminary questions, but also in indicating the line along which the most productive results will probably be found.

The lower structural grades of steel, being little else than "melted wrought iron," can be welded with almost, or quite as much, facility as wrought iron, while its superior ductility and perfect homogeneity make it especially adapted to such processes as upsetting end, hence to the production of eye-bars. It is very true that these observations now apply only to those low grades of steel that exceed wrought iron in ultimate tensile resistance by at most 25 per cent., and that exercise of increased care and skill are required, but the latter circumstances are always necessary concomitants of any advance, and there is no reason to doubt that an enlarged experience will result in the employment of a metal of constantly increasing ultimate tensile resistance. Any process or operation which may obviate the final step of annealing steel eye bars will remove the last sensible (though not serious) obstacle to their general use.

The employment of operations less disturbing to the molecular arrangement, in the production of steel compression members, facilitates the use of a grade of metal giving a much higher resistance than that of which the eye bars are made. It is yet a question just how much higher steel may be used in compression than in tension members; but it is already certain that a higher grade may be used, and hence that the advantage in steel columns is correspondingly greater.

Many tests have already been made on steel angle struts, though few on composite columns; enough has been done, however, to show the superiority of steel struts over those of iron. The freedom of steel from slag and its perfect homogeneity give a greater range to the shapes in which it can be produced, and render it capable of resisting severer duties with less fatigue. The possession by steel of a greater number of better structural qualities gives certainty to the early solution of the more fundamental and important problems involved in the general introduction of that metal.

It is evident, therefore, that in some respects, at least, the approaching use of steel forces bridge building into somewhat of a transition state, in which will arise, or rather have arisen, questions affecting not only the proportions of cross-sectional areas of main truss members, but others depending on the design of details.

While, however, the preceding questions embrace difficulties of no ordinary character, they are subject to a relatively easy treatment, since the effect of a variation in attending circumstances may be determined both in quantity and quality, there are, on the other hand, numerous questions arising from the manner of application and distribution of the moving load which, on account of the varying conditions of action, are essentially indeterminate. The general question of moving loads was originally decided in a very simple and summary manner by considering it equivalent to double the amount of static load. Such a method is certainly simple enough, but does not more than very loosely represent the true action of the moving load; it would only accurately represent it if the load were applied instantaneously over the *whole* bridge without the slightest shock. But even under the most rapidly moving train this can scarcely be considered approximately true, though such an assumption would

be more reasonable in connection with the floor system.

Again the principle just given is based on the supposition that the load is not only applied instantaneously, but allowed to remain applied in one position long enough to allow the structure to take a deflection double that due to a static load just equal in amount to the moving. As a matter of fact, the moving load remains in one position an indefinitely short time, and, in addition to that fact, the members of any bridge possess an amount of inertia that would materially delay (in time) the deflection, even if all other circumstances were favorable. If the bridge is of long span so that the fixed weight (*i. e.*, of structure) becomes proportionately large, the inertia will militate with increased intensity against the conditions requisite for the support of the old hypothesis. The definite duration of time in application of load at any one point, which is the essential basis of that hypothesis, seems to have been strangely overlooked by confusing rapidity of motion with suddenness of application; conditions neither alike in direction nor similar in nature. It may, indeed, be shown that rapidity in movement, *per se*, may relieve stress rather than increase it, for the simple reason that when the load is "suddenly" applied at any one point, it is just as "suddenly" removed.

These considerations will gain force in connection with some simple computations based on the movement of rapidly passing loads. If a train moves at the rate of 60 miles per hour, it will pass over 88 feet in one second, or about 10½ inches in one one-hundredth of a second. During the same length of time a heavy body will fall a little more than one-sixty-fourth of an inch vertically. In other words, if there happens, from any cause, to be a descending slope in the track of 1 in 670 for a distance of 10½ inches, the approach thereto being level, a train moving at the rate of sixty miles an hour would produce no pressure on the track over that space. Now, if the hypothesis of a "suddenly applied load" has any application whatever to one in rapid horizontal motion, the accuracy of its application ought to increase with the rapidity of the motion. But if, in sixty miles an hour, the load moves 10½ inches in less time than a deflection of one-sixty-fourth of an inch can take place, it is readily seen how thoroughly erroneous must be the hypothesis which assumes no motion in a horizontal direction, while a deflection of one to six inches, according to length of span, may take place. The engineer, therefore, must look for another origin than that of "suddenly applied loads" for the known destructive effects of a rapidly moving load.

In all the observations on this question it has been assumed (as indeed was assumed in the old hypothesis) that the track is absolutely smooth and that no shock takes place. But let it be considered what occurs at the post of the 10½-inch incline. The load has fallen one-sixty-fourth of an inch, and if the track is again level, the moving load has become also a falling load, and shock will take place. These effects will be aggravated if the slope continues, or is steeper, or, again, if at its foot it joins an ascending slope. Even if a track is laid with the greatest skill, and maintained with the most watchful care, these slight elevations and depressions, inappreciable as they may be to the unaided eye, must exist to a greater or less extent. If to these we add the shocks and hammerings which arise at rail joints, it will be difficult neither to discover the origin of shocks and vibrations, nor to appreciate the wisdom of the ample margins of safety employed in structural design. Those shocks will not coincide in time and position for each rail, and the result will be violent movements both lateral and vertical, originating vibrations transverse and longitudinal in direction as well as vertical. Any longitudinal effects will be very materially aggravated by all train movements, save those under just enough steam to produce uniform motion.

A heavy consolidation locomotive may exert a tractive force on the rails of twenty-five to thirty thousand pounds, the greater part or all of which will be resisted by the floor system of a bridge if train motion is begun on such a structure, and a still greater force may be called into action by the air brakes of a quickly stopping train. It is erroneous to imagine, as is frequently done, that a train passing a bridge without steam exerts no tractive force. The rolling and axle friction always exist as a force opposed to the actual motion, and if the

locomotive takes no steam on a level, this force acts to drive the rails ahead, and is only neutralized by just enough steam to keep the train in uniform motion. This effect may easily be observed by measuring the movement of a marked point on a rail under a train moving by its inertia only.

All these effects are produced by the external action of the moving load, but there are others of a different character, possibly little or no less severe in their action. These result from the rapid rotation of the counterweight of a locomotive driving wheel combined with the vertical component of the thrust or pull on the main connecting rod. The first of these is most active in express locomotives, and the latter in those engines engaged in the heaviest traffic. On account of the low speed of the latter class, both effects will act most destructively under locomotives designed for high speed.

Mr. J. W. Cloud, of Altoona, Pa., has shown that the counterweight (weighing 300 lbs.) on the driving wheel of a "Class B" locomotive on the Pennsylvania R. R., running at the rate of fifty miles per hour, will exert a "blow" every fourteen seconds of 6,260 pounds above the regular moving load for a single rail, and, of course, an equal amount below it, making the total blow 12,520 pounds. Under the same locomotive the vertical component of the thrust of the main connecting rod, with an initial steam pressure of 110 pounds, and cut off at one-half, inflicts a "blow" of nearly half the magnitude of that of the counterweight, and with the same frequency. The resultant of these two effects amounts to a "blow" of about 12,500 pounds on each track for each front and rear driver, every fourteen seconds. The variation of vertical thrust in the main connecting rod involves a correspondingly periodical redistribution of the track loads, and as none of these effects coincide on the two sides of the locomotive there is generated a tendency to "roll" both transversely and longitudinally, the latter of which frequently produces very considerable motion in the engine.

At first sight it would appear that the life of a structure must be short under such fatiguing duty. It is, however, scarcely accurate to call these effects "blows"; in reality they are not "blows," but rapid variations of force taking place with the locomotive only, and never repeated at the same point.

Even at 60 miles per hour it would require eleven seconds to produce one of those effects in the engine assumed, which is certainly not identical with a "blow" consuming perhaps one-ten-thousandth part of that time; nor is it a "shock."

While, therefore, it is absolutely necessary to recognize the fatigue of the resulting vibrations, particularly in the floor system, where it is more severe than in the trusses, in the determination of the proper working stresses, it is probable that a slight relaxation in the maintenance of a high degree of track excellence would originate shocks far more destructive in character.

The qualitative analysis of any of these influences, whether of moving load, shocks, counterweight, or main rod, when once recognized, presents no particular difficulty. Unfortunately, however, the engineer needs not only the quality but the quantity of the destructive forces, and the presence of the uncertain element of time renders the exact solution of these questions impossible, and a resort to the judgment, tempered by experience, the only possible method of treatment. The *raison d'etre* of our ample margins of safety for moving loads, is then sufficiently clear, though we are unable to express it in mechanical units.

The recognition of wind pressure as an agent at times actively aiding the destruction of bridges is a late matter in structural design, but the derailed locomotive no less than the railway bridge destroyed by the unaided wind demonstrate that the recognition is most timely. These extremely violent pressures are happily of rare occurrence, and the ordinary intensities are easily provided against in all usual designs.

A matter of the greatest practical importance is the distribution of the moving load, and that importance is intensified by the constantly increasing weight of locomotives with their tenders. The old method of a certain amount of uniform load per lineal foot of track served a good purpose in its day, but the requirements of the present time make it necessary that the imaginary uniform locomotive load should give way to the ac-

tual loads applied at wheel points. This observation bears with particular force on the design of the floor system and those web members near the center of the span, especially when it is borne in mind that these are the members subject to the greatest fatigue. It is now clearly recognized that in order to find a uniform load equivalent to a given number of concentrations, so far as chord or flange stress is concerned, the greatest moment due to the concentration loads must first be found, and then a uniform load which will produce the same moment determined; this latter will be the equivalent uniform load. The table on next column shows the results of this process applied to a consolidation engine and tender weighing 171,000 pounds, with 96,000 pounds on a driving-wheel base of 14 feet 9 inches.

Above 55 feet the equivalent uniform load per lineal foot will slowly decrease until it reaches a value of about 3,200 lbs. for 100 feet and over, *i. e.*, supposing the moving load to consist of a train of such locomotives.

Span in feet.	Equiv. uniform load in lbs. per lin. foot.	Span in feet.	Equiv. uniform load in lbs. per lin. foot.
55	3750	25	4838
50	3866	20	5137
45	4004	15	5760
40	4242	12	6000
35	4336	10	5766
30	4572	5	9600

It is very clear, therefore, why the intensity of the moving load should increase as the length of span decreases, and it is equally clear that in consequence of the greater relative value of the moving load, stresses as compared with those of the fixed load, no less than the intensified effects of shocks, vibrations, etc., the fatigue of the metal in short spans will be much greater than in long ones, and consequently that the working stresses per unit of area should be correspondingly less in the former.

ON THE FRITTS SELENIUM CELLS AND BATTERIES.

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From the Proceedings of the American Association for the Advancement of Science.

In all previous cells, so far as I am aware, the two portions or parts of the selenium, at which the current enters and leaves it, have been in substantially the same electrical state or condition. Furthermore, the paths of the current and of the light have been transverse to each other, so that the two forces partially neutralize each other in their action upon the selenium. Lastly, the current flows through not only the surface layer which is acted upon by the light, but also through the portion which is underneath and not affected thereby, and which therefore detracts from the actual effect of the light upon the selenium at the surface.

My form of cell is a radical departure from all previous methods of employing selenium, in all of these respects. In the first place, I form the selenium in very thin plates, and polarize them, so that the opposite faces have different electrical states or properties. This I do by melting it upon a plate of metal with which it will form a sort of chemical combination, sufficient, at least, to cause the selenium to adhere and make a good electrical connection with it. The other surface of the selenium is not so united or combined, but is left in a free state, and a conductor is subsequently applied over it by simple contact or pressure.

During the process of melting and crystallizing, the selenium is compressed between the metal plate upon which it is melted and another plate of steel or other substance with which it will not combine. Thus, by the simultaneous application and action of heat, pressure, chemical affinity and crystallization, it is formed into a sheet of granular selenium, uniformly polarized throughout, and having its two surfaces in opposite phases as regards its molecular arrangement. The non-adherent plate being removed after the cell has become cool, I then cover that surface with a *transparent conductor of electricity*, which may be a thin film of gold leaf. Platinum, silver, or

other suitable material may also be employed. The whole surface of the selenium is therefore covered with a good electrical conductor, yet is practically bare to the light, which passes through the conductor to the selenium underneath. My standard size of cell has about two by two and a-half inches of surface, with a thickness of $\frac{1}{1000}$ to $\frac{5}{1000}$ inch of selenium, but the cells can, of course, be made of any size or form. A great advantage of this arrangement consists in the fact that it enables me to apply the current and the light to the selenium in the same plane or general direction, instead of transversely to each other, as heretofore done, so that I can cause the two influences to either coincide in direction and action, or to act upon opposite faces of the selenium and oppose each other, according to the effect desired.

By virtue of the process and arrangement described, my cells have a number of remarkable properties, among which are the following:

1. *Their sensitiveness to light* is much greater than ever before known. The most sensitive cell ever produced, previous to my investigations, was one made by Dr. Werner Siemens, which was 14.8 times as conductive in sunlight as in dark. In Table A, I give results obtained from a number of my cells.

TABLE A.

SENSITIVENESS TO LIGHT.

Selenium cell.	Battery power.	Resistance in dark.	Resistance in sunlight.	Ratio.
No. 22	5 elements	39,000 ohms	340 ohms	114 to 1
" 23*	5 "	14,000 "	170 "	82.3 " 1
" 24†	5 "	648,000 "	2,400 "	270 " 1
" 25	5 "	180,000 "	930 "	196.5 " 1
" 26	5 "	135,000 "	710 "	190 " 1
" 107	5 "	118,000 "	740 "	159 " 1
" 108	5 "	200,000 "	900 "	222 " 1
" 122	5 "	56,000 "	220 "	254.5 " 1
" 129*	5 "	200,000 "	940 "	212 " 1
" 137	5 "	108,000 "	320 "	337.5 " 1

* Cells No. 23 and No. 129 are now in possession of Prof. W. Grylls Adams, of King's College, London; Dr Werner Siemens has No. 25, and Prof. George F. Barker, of Philadelphia, has No. 26.
† No. 24 was measured with a bridge multiplier of 6 to 1.

It will be observed that I have produced one cell which was 337.5 times as conductive in hazy sunlight as in dark. The tremendous change of resistance involved in the expression "337.5 times" may perhaps be more fully realized by saying that 99.704 *per cent.* of the resistance had disappeared temporarily, under the joint action of light and electricity, so that there remained *less than* $\frac{3}{10}$ *of* 1 *per cent.* of the original resistance of the selenium in dark.

In order to obtain these high results, the cells must be protected from light when not in use. The resistance is first measured while the cell is still in total darkness. It is then exposed to sunlight and again measured. It is also necessary to send the current in at the gold electrode or face, as the cell is much less sensitive to light when the light acts upon one surface of the selenium, and the current enters at the opposite surface. When the two influences, the light and the current, act through the gold in conjunction, their forces are united: and as every atom of the selenium is affected by the light, owing to the extreme thinness of the plate, we have the full effect shown in the measurements.

Cells which are sensitive to light improve by being used daily, and their sensitiveness becomes less if they are laid aside and not used for a considerable length of time, especially if allowed to become overheated. They should be kept cool, and exposed to light frequently, whether they are used or not.

Mode of measuring cells.—So great is the sensitiveness of these cells to external influences, that it is necessary to adopt some particular system in measur-

ing their resistance, and to adhere strictly to that system, as every change in the method of measurement produces a difference in the result, and the different measurements would not be comparable with each other. The reason for this will be explained presently.

The system I have adopted is the Wheatstone's bridge arrangement, with equal sides, never using multipliers, except for some experimental purpose. In each multiplier wire I have 500 ohms resistance. When the bridge is balanced, one-half of the current flows through the cell and acts upon the selenium. Between the bridge and the cell is a reversing switch so that the current can be reversed through the cell without changing its course through the bridge. A Bradley tangent galvanometer is used, employing the coil of 160 ohms resistance. The Leclanché battery is exclusively used in measurements for comparison.

2. *The kind of battery employed* has a marked effect upon the sensitiveness to light, which is largely reduced or entirely destroyed when the bichromate battery is used. The same cells again become extremely sensitive with the Leclanché battery. We might naturally expect that a change in the current employed would cause a change in the *resistance* of a cell, but it is not clear how or why it should affect the *sensitiveness of selenium to light.*

"If one kind of battery current destroys its sensitiveness, may we not suppose that another kind might increase its sensitiveness? Although the Leclanché has operated well, some other may operate still better, and by its special fitness for use on selenium cells, may intensify their actions, and so bring to light other properties yet unthought of. Is not here a promising field for experiment, in testing the various forms of battery already known, or even devising some new form especially adapted to the needs and peculiarities of selenium cells?

One year ago I made the foregoing suggestion in a paper on "A new Form of Selenium Cell," presented before this Association at Minneapolis. I am now at liberty to state that my photo-electric battery, presently to be described, marks an advance in the direction indicated. The current from this battery increases the sensitiveness of the cells to light, and also to reversal of current. One cell, whose highest ratio in light was about 83 to 1, with the Leclanché battery, when measured with my battery, gave a ratio of 120 to 1. It seems to make the resistance of the cell both higher in dark and lower in sunlight than with the Leclanché battery. But the field is yet open to others, for the discovery of a battery which may be still better for use with selenium cells.

3. *The two surfaces of the selenium act differently towards currents* sent into them from the contiguous conductors. One surface offers a higher resistance to the current than the other. The former I utilize as the anode surface, as I have found that the cell is more sensitive to light when the current enters at that surface, which is ordinarily the one covered by the gold or other transparent conductor. Some cells have this property but feebly developed; but in one instance the resistance offered to the current by the anode surface was 256 times as high as that offered by the cathode surface to the same current. In the majority of cases, however, the ratio

TABLE B.

SENSITIVENESS TO REVERSAL OF DIRECTION OF CURRENT.

No. of cell.	Battery.	Resistance. "gold anode"	Resistance, "gold cathode."	Ratio.
½ inch sq., No. 4	5 elements.	20,000 ohms.	1,000 ohms.	20 to 1
" " " 3	*Se.* cell.	6,500 "	400 "	16.2 " 1
Full size, No. 13	1 element.	9,000 "	800 "	11.2 " 1
" " 14	5 elements	2,440 "	130 "	18 " 1
" " 15	5 "	4,640 "	210 "	22 " 1
" " 27	5 "	6,900 "	440 "	16 " 1
" " 126	1 element.	5,000 "	330 "	15 " 1

does not exceed ten times. Table B gives some recent results.

The direction of the current is always indicated by stating the position of the gold electrode—by the terms "gold anode" and "gold cathode." The above measurements were made in dark.

4. *Sensitiveness to change of battery power.*—My cells are extremely sensitive to any change in the strength or character of the current flowing through them, which is shown by a corresponding change in the resistance of the cell. I can, therefore, vary the resistance of one of my cells in many ways, and the following may be specified:

(*a*) By changing the potential or electromotive force of the current through the cell.

(*b*) By changing the "quantity" of the battery or current.

(*c*) By putting more or less resistance in the circuit.

(*d*) By dividing the current by one or more branch circuits or shunts around the cell.

(*e*) By varying the resistance in any or all of said circuits.

A cell whose resistance becomes greater as the battery power becomes greater, and *vice versa*, I call an "L. B." cell, signifying *Like the Battery power.* A "U. B. cell" is one whose resistance becomes greater as the battery power (or strength of current) becomes less, and *vice versa*, being *Unlike the Battery power* or current strength.

These changes of resistance are not due to heating of the conductor or the selenium, and the following instance will illustrate this. I have one cell in which the selenium had about one-fourth inch square of surface melted on a brass block one inch thick. This cell measured, with 25 elements of Leclanché, 40,000 ohms. On changing the battery to 5 elements, the resistance fell instantly to 30 ohms, and there remained. On again using the current from 25 elements, the resistance instantly returned to 40,000 ohms. Had these results been due in any degree to heating, the resistance would have changed gradually as the heat became communicated to the brass—whereas no such change occurred, the resistances being absolutely steady. Moreover, even the fusion of the selenium would not produce any such change.

The "U B" property does not ordinarily change the resistance of the cell to exceed ten times, *i.e.*, the resistance with a weak current will not be over ten times as high as with a strong one. But I have developed the "L B" property to a far higher degree. Table C gives some recent results obtained with L B cells, including one whose resistance, with 25 elements Leclanché, was 11,381 times as high as with 8 elements, and which, after standing steadily at 123 ohms (and then at 325 ohms with 1 element), on receiving the current from 25 elements, again returned to its previous figure of 1,400,000 ohms.

TABLE C.

SENSITIVENESS TO CHANGE OF BATTERY POWER.

No. of cell.	Resistance with 25 elements.	Resistance with 5 elements.	Ratio of change.
¼ inch sq., No. 1	40,000 ohms	30 ohms	1,333 to 1
" " 2	13,000 "	40 "	325 " 1
" " 1	1,400,000 "	123* "	11,381 " 1
" " 2	500,000 "	62 "	8,064 " 1
" " 5	3,500 "	21 "	167 " 1
Full size, No. 81	68,000 "	121 "	561 " 1
" " 82	9,000 "	64 "	140 " 1
" " 83	17,300 "	74 "	233 " 1
" " 119	35,600 "	19 "	1,894 " 1

* This measurement was obtained with 8 elements.

The results in the table were obtained by changing the strength of current by throwing in more or less of the battery. Like results can be obtained by varying

the current through the cell by any of the other methods before specified. The above measurements were in dark.

5. *Dual state of selenium.*—My cells, when first made, seem to have two states or conditions. In one, their resistance is very low, in the other it is high. When in the low state they are usually not very sensitive, in any respect. I therefore raise the resistance by sending an intermittent or an alternating current through the cells, and in their new condition they at once become extremely sensitive to light, currents, and other influences. In some cases they drop to the low state again, and require to be again brought up until, after a number of such treatments, they remain in the sensitive state. Occasionally a cell will persist in remaining in the insensitive state. The before-mentioned treatment raises it up for a moment, but, before the bridge can be balanced and the resistance measured, it again drops into the low or insensitive state. Some cells have been thus stimulated into the high or sensitive state repeatedly, and every means used to make them stay there, but without avail; and they have had to be laid aside as intractable.

In the earlier stages of my investigations, before the discovery of this dual state and the method of changing a cell from the insensitive to the sensitive condition, hundreds of cells were made, finished, and tested, only to be then ruthlessly destroyed and melted over, under the impression that they were worthless. Now, I consider nothing worthless, but expect sooner or later to make every cell useful for one purpose or another.

The most singular part of this phenomenon is the wide difference in the resistance of the cells in the two states. In the low state it may be a few ohms, or even a few hundredths of an ohm. In the high state, it is the normal working resistance of the cell, usually between 5,000 and 200,000 ohms, but is often up among the millions. The spectacle of a little selenium being stimulated, by a few interruptions of the current through it, into changing its resistance from a fraction of an ohm up to a million or several millions of ohms, and repeatedly and instantly changing back and forth, up and down, through such a wide range —we might almost say, changing from zero to infinity, and the reverse, instantly —is one which suggests some very far-reaching inquiries to the electrician and the physicist. What is the nature of electrical conductivity or resistance, and how is it so greatly and so suddenly changed?

6. *Radio-electric current generators.*—My cells can be so treated that they will generate a current by simple exposure to light or heat. The light, for instance, passes through the gold and acts upon its junction with the selenium, developing an electromotive force which results in a current proceeding from the metal back, through the external circuit, to the gold in front, thus forming a photo-electric dry pile or battery. It should preferably be protected from overheating by an alum-water cell or other well-known means.

The current thus produced is radiant energy converted into electrical energy directly and without chemical action, and flowing in the same direction as the original radiant energy, which thus continues its course, but through a new conducting medium suited to its present form. This current is continuous, constant, and of considerable electromotive force. A number of cells can be arranged in multiple arc or in series, like any other battery. The current appears instantly when the light is thrown upon the cell and ceases instantly when the light is shut off. If the light is varied properly, by any suitable means, a telephonic or other corresponding current is produced, which can be utilized by any suitable apparatus, thus requiring no battery but the selenium cell itself. The strength of the current varies with the amount of light on the cell, and with the extent of the surface which is lighted.

I produce current not only by exposure to sunlight, but also to dim diffused daylight, and even to lamplight. I have used this current for actual working purposes—among others, for measuring the resistance of other selenium cells, with the usual Wheatstone's bridge arrangement, and for telephonic and similar purposes. Its use for photometric purposes and in current-regulators, will be mentioned farther on. It is undoubtedly available for all uses for which other battery currents are em-

ployed, and I regard it as the most constant, convenient, lasting, readily used and easily managed pile or battery of which I have any knowledge. On the commercial scale, it could be produced very cheaply, and its use is attended by no expense, inasmuch as no liquids or chemicals are used, the whole cell being of solid metal with a glass in front, for protection against moisture and dust. It can be transported or carried around as easily and safely as an electro-magnet, and as easily connected in a circuit for use wherever required. The current, if not wanted immediately, can either be "stored" where produced, in storage batteries of improved construction devised by me, or transmitted over suitable conductors to a distance, and there used, or stored as usual till required.

7. *Singing and Speaking Cells.*—When a current of electricity flowing through one of my selenium cells is rapidly interrupted, a sound is given out by the cell, and that sound is the tone having the same number of air vibrations per second as the number of interruptions in the current. The strength of the sound appears to be independent of the direction of the current through the cell. It is produced on the face of the cell, no sound being audible from the back of the cell. An alternating current also produces a sound corresponding to the number of changes of direction. Experiments also show that, if a telephonically undulating current is passed through the cell, it will give out the speech or other sound corresponding to the undulations of the current—and, furthermore, that cell will sing or speak in like manner, without the use of a current, if a suitably varied light is thrown upon it while in closed circuit.

My experiments having been devoted especially to those branches of the subject which promised to be more immediately practically valuable, I have not pursued this inquiry very far, and offer it for your consideration as being not only interesting, but possibly worthy of full investigation.

GENERAL OBSERVATIONS ON THE PROPERTIES OF CELLS.

From the number of different properties possessed by my cells, it might be anticipated that the different combinations of those properties would result in cells having every variety of action. This is found to be the case. As a general rule, the cells are noteworthy in one respect only. Thus, if a cell is extremely sensitive to light, it may not be be specially remarkable in other respects. As a matter of fact, however, the cells most sensitive to light are also "U B cells."

The property of sensitiveness to light is independent of the power to generate current by exposure to light—the best current-generating cells being only very moderately sensitive to light, and some of the most sensitive cells generate scarcely any current at all. Current-generating cells are, almost without exception, "U B cells;" and the best current-generating cells are strongly polarized, showing a considerable change of resistance by reversing the direction of a current through them; and they are also strong "anode cells," *i. e.*, the surface next to the gold offers a higher resistance to a battery current than the other surface of the selenium does. The power to generate a current is temporarily weakened by sending a battery current through the cell while exposed to light, in either direction. The current generated by exposure to light is also weakened by warming the cell, unless the cell is arranged for producing current by exposure to heat.

The properties of sensitiveness to light and to change of battery power are independent of each other, as I have cells which are sensitive to change of current but absolutely insensitive to light—their resistance remaining exactly the same whether the cells are in darkness or in sunlight. I also have cells which are sensitive to light, but are unaffected by change of battery power, or by reversing the direction of the current through them.

The sensitiveness to change of battery power is also independent of the sensitiveness to reversal of direction of the current. Among the best "L B cells," some are "anode cells" and others are "cathode cells," while still others are absolutely insensitive to reversal of current, or to the action of light.

Constancy of the Resistance.—A noticeable point in my cells is the remarkable constancy of the resistance in sun-

light. Allowing for differences in the temperature, the currents, and the light, at different times, the resistance of a cell in sunlight will remain practically constant during months of use and experiments, although during that time the treatments received may have varied the resistance in dark hundreds of thousands of ohms—sometimes carrying it up, and at others carrying it down again, perhaps scores of times, until it is "matured" or reaches the condition in which its resistance becomes constant.

As has already been stated, the sensitiveness of a cell to light is increased by proper usage. This increased sensitiveness is shown, not by a lowered resistance in light, but by an increased resistance in dark. This change in the cell goes on, more or less rapidly, according as it is retarded or favored by the treatment it receives, until a maximum is reached, after which the resistance remains practically constant in both light and dark, and the cell is then "matured" or finished. The resistance in dark may now be 50 or even 100 times as high as when the cell was first made, yet, whenever exposed to sunlight it promptly shows the same resistance that it did in the beginning. The various treatments, and even accidents, through which it has passed in the meantime, seem not to have stirred its molecular arrangement under the action of light, but to have expended their forces in modifying the positions which the molecules must normally assume in darkness.

Practical Applications.—There are many peculiarities of action occasionally found, and the causes of such actions are not always discernible. In practice, I have been accustomed to find the peculiarities and weaknesses of each cell by trial, developing its strongest properties and avoiding its weaknesses, until, when the cell is finished, it has a definite and known character, and is fitted for certain uses and a certain line of treatment, which should not be departed from, as it will be at the risk of temporarily disabling it. In consequence of the time and labor expended in making cells, in the small way, testing, repairing damages done during experiments, etc., the cost of the cells now is unavoidably rather high. But if made in a commercial way, all this would be reduced to a system, and the cost would be small. I may say here that I do not make cells for sale.

The applications or uses for these cells are almost innumerable, embracing every branch of electrical science, especially telegraphy, telephony and electric lighting, but I refrain from naming them. I may be permitted, however, to lay before you two applications, because they are of such general scientific interest. The first is my

Photometer.—The light to be measured is caused to shine upon a photo-electric current-generating cell, and the current thus produced flows through a galvanometric coil in circuit, whose index indicates upon its scale the intensity of the light. The scale may be calibrated by means of standard candles, and the deflections of the index will then give absolute readings showing the candle-power of the light being tested. Or, the current produced by that light and that produced by the standard candle may be compared, according to any of the known ways of arranging and comparing different lights—the cell being lastly exposed alternately to the two lights, to see if the index gives exactly the same deflection with each light.

This arrangement leaves untouched the old difficulty in photometry, that arising from the different *colors* of different lights. I propose to obviate that difficulty in the following manner. As is well known, gold transmits the green rays, silver the blue rays, and so on; therefore, a cell faced with gold will be acted upon by the green rays, one faced with silver by the blue rays, etc. Now if we construct three cells (or any other number) so faced that the three, collectively, will be acted upon by all the colors, and arrange them around the light to be tested, at equal distances therefrom, each cell will produce a current corresponding to the colored rays suited to it, and all together will produce a current corresponding to all the rays omitted by the light, no matter what the proportions of the different colors may be. The three currents may act upon the same index, but each should have its own coil, not only for the sake of being able to join or to isolate their influences upon the index, but also to avoid the resistances of the other cells. If a solid transparent conductor of electricity could be found

which could be thick enough for practical use and yet would transmit all the rays perfectly, *i. e.*, transmit white light unchanged, that would be still better. I have not yet found a satisfactory conductor of that kind, but I think the plan stated will answer the same purpose. This portion of my system I have not practically tested, but it appears to me to give good promise of removing the color stumbling-block which has so long defied all efforts to remove it, and I therefore offer it for your consideration.

Photo-Electric Regulator.—My regulator consists of a current-generating cell arranged in front of a light, say, an electric lamp whose light represents the varying strength of the current which supports it. The current produced in the cell by this light flows through an electro-magnetic apparatus by means of which mechanical movement is produced, and this motion is utilized for changing resistances, actuating a valve, rotating brushes, moving switches, levers, or other devices. This has been constructed on a small scale and operates well, and I think it is destined to be largely used, as a most sensitive, simple and perfect regulator for currents, lights, dynamos, motors, etc., etc., whether large or small.

In Conclusion, I would say that the investigation of the physical properties of selenium still offers a rare opportunity for making very important discoveries. But candor compels me to add that whoever undertakes the work will find it neither an easy nor a short one. My own experience would enable me to describe to you scores of curious experiments and still more curious and suggestive results, but lack of time prevents my giving more than this very incomplete outline of my discoveries.

HYDRAULIC PROPULSION.

By SYDNEY WALKER BARNABY, Assoc. M. Inst. C. E.

Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Sir J. W. Bazalgette, C. B., President, said, in proposing a vote of thanks to Mr. Barnaby for his interesting paper, that it was just thirty years since the subject was discussed at the Institution. The record of that discussion was very meager, and perhaps it was well that it was so, for report stated that the discussion was exceedingly animated, and somewhat personal, and that the language used on that occasion was not altogether parliamentary. He was sure that on the present occasion the discussion would be animated; he was equally sure that it would be conducted without any personal feeling, in a perfectly "parliamentary" manner; and he could also promise that a true and complete record of the discussion would be given.

Mr. Sydney W. Barnaby asked permission to call attention to a diagram which had not been alluded to in the paper, but was illustrated by a model on the table. It represented a small steam launch driven by what he had called a screw-turbine propeller. It had been already described at the Institution and elsewhere, and he only alluded to it because it was really the outcome of the hydraulic-boat. It had been pointed out in the paper that there were four features in the Ruthven form of pump which militated against its success as a propeller. In the first place, there was the difficulty of getting the water through the bottom of the boat and into the pump without checking the velocity it already had relative to the vessel. In the second place, there was the disadvantage of the extra weight of machinery involved by the necessity of carrying all the water acted upon. Thirdly and fourthly, there was the loss by friction and by bends in the passages. In working out the hydraulic-boat, it occurred to Mr. Thornycroft that if the turbine could be put outside the boat and under the bottom, the first two causes of loss would be avoided. It was also clear that if the water

could be made to flow axially through the turbine instead of centrifugally, as in the Ruthven pump, no pipes would be needed, and there would be no loss by friction or by bends. This was what had really been done in the boat, illustrated by the diagram and model, to which he had referred. It might be said that it was not a turbine, but it would certainly be an abuse of language to call it a screw. He thought it might be fairly described as a screw-turbine propeller. Another point in its favor was that at least as light a draught (and in some cases a lighter) could be obtained as with an actual centrifugal pump inside the boat. In the second class torpedo-boat, if a screw-turbine propeller had been used, its diameter would have required to have been 21 inches only, and the draught could have been reduced from 2 feet 6 inches, which it was when fitted with the turbine, to 2 feet 2 inches. Therefore, the principal point which Lord Dufferin's Committee had mentioned as a reason for a further trial of the turbine propeller had been secured by a simpler method avoiding a great many of its disadvantages. The steering power also was very superior with the turbine underneath the bottom and near the end to what it was with the nozzles in the ordinary hydraulic-boat, the reason being that almost the full power of the engines was used in both cases; but in the steam-launch (Plate 1) it acted upon a leverage of half the length of the boat instead of a leverage of half the breadth, as in the case of the hydraulic-boat. He thought it might therefore be taken that that was at any rate one solution of the problem of hydraulic propulsion.

Mr. M. W. Ruthven said that the author had taken some trouble to show that great losses were sustained by the hydraulic mode of propulsion; the question, however, was not how much loss was due to the hydraulic method, but was the loss less or greater than by the screw? The safest way to get at that was by a comparison of the results. It

Viper Class.		Waterwitch.		Philomel Class.	
9th May, 1866	274.8	Coefficient from $\frac{v^3 \times M S}{I.HP.}$		14th March, 1868	302.0
18th " "	297.2	1st Jan., 1867	342.7	8th May, "	316.0
31st " "	328.0	17th " "	327.7	20th March, "	299.0
5th Nov., "	317.7	9th Aug., "	351.9	16th April, "	274.0
4th June, "	321.0	28th " "	328 7	14th July, "	329.0
5th " "	335.9	3d Sept., "	303.3	20th March, "	279.0
2d Aug., 1867	379.9	12th Oct., "	357.8	20th May, "	311.0
5th " "	437.6		6)2012.1	19th Nov., "	294.0
11th Oct., "	424.8		335.3	12th May, "	311.0
	9)3116.9		12.2	27th Oct., "	272 0
	346.3				10)2987.0
	3.2				298.7

was for this purpose that the trials of the Waterwitch, Vixen and Viper, had been undertaken, though a comparison might also be made with the screw in other ships. The author had chosen to compare the Waterwitch only with the Viper, and with a particular trial; but as the results of the trials of the Viper, and Vixen varied nearly 60 per cent., the only approach to accuracy would be the mean of all the trials of both ships, compared with the mean of all the trials of the Waterwitch. When that was done it would be found that the efficiency of the hydraulic propeller was within 3 per cent. of the screw propeller in the Viper and Vixen; but as the results of the trials of those vessels varied so much they did not form a satisfactory comparison. There was, however, a class of vessels called the Philomel class, with similar engine and boiler-power to that of the Viper and Vixen, whose performance varied less. Now comparing the coefficients of this superior class of ships with the coefficients of the Waterwitch, it would be found that the hydraulic-propeller in the Waterwitch was more than 12 per cent. better than the screw-propeller in the Philomel class. In Table 1 the coefficient of the hydraulic-propeller in the Waterwitch was put as 116.9,

while that of the Swedish boat was 52.5, showing that the former was 122 per cent. better. The Thornycroft was stated to be 72, giving the hydraulic-propeller in the Waterwitch a superiority of 62 per cent. In Table II., however, this seemed reversed, for the total efficiency of the hydraulic-propeller in the Waterwitch was put as 0.180, in the Swedish at 0.214, and in the Thornycroft boat at 0.250. Now the use of the column of coefficients was to afford a comparison of efficiency, and this being so, that column in Table 2, showing a total efficiency, must keep the same relations as in Table 1. Mr. Froude, he believed, found that a good screw had a total efficiency of 0.40. Taking this to be correct, and seeing that the hydraulic-propeller in the Waterwitch was superior to the screw by 12 per cent., its total efficiency was thus proved to be 0.44; and unless the author could show that the total efficiency of the screw-propeller was less than 0.18, he must be wrong in putting the total efficiency of the hydraulic-propeller in the Waterwitch at that figure.

Captain Heathorn observed that five years ago Mr. Ruthven had given him copies of the tables of the various trials to which he had referred, and he had himself given copies to many members of the Institution. He only hoped that every possible experiment would be tried to see if greater velocities could not be obtained. Mr. Thornycroft had invented something like a Giffard's injector, and had obtained a good result. The force was a flow of water instead of steam; surface friction was at work and produced the required result. About eleven months before his recognition of surface friction as a means of propulsive effort, Captain Heathorn had tried experiments with many kinds of orifices, and found that the force of a stream of water, from a pipe under water, in the still water surrounding was, when once the flow began, not so much dependent on its sectional area, as on the surface contacts; and that if the area of contact was larger in proportion to the area of the section of the flow from a round pipe in one case and one of another form in the other, greater surfaces set up greater friction, greater speed resulted and less water was used. The Admiralty had put him in communication with the late Mr. Froude, who, in answer to a letter of the 20th of March, 1878, said that the proposition which he had laid down as the basis of his view was one with which Mr. Froude was familiar, and he went on to ask in what way Captain Heathorn proposed to apply it to the propulsion of ships. His answer was: Contract the orifices or jets of H. M. S. Waterwitch to a certain form, and do the same to the exit ends of the screw channels of H. M. S. Bruiser. Screw channels, he believed, were invented by the late Mr. Griffith. Now that the Admiralty had a boat he hoped that they would persevere, and find out what hydraulic power really was.

Admiral Selwyn considered the paper an important contribution to the history of the subject. If any means could be adopted, even at the sacrifice of speed, to prevent ships and their crews going suddenly to the bottom by the impact of a torpedo or a ram, he was sure Englishmen would advocate spending a little more money for the extra power required to give the necessary velocity, and at the same time secure the other advantages. He doubted the accuracy of the statement in the paper that the principles underlying the subject were perfectly understood; he thought that, otherwise, none of the experiments would have been made on the basis on which they had been carried out in the Waterwitch and the other similar vessels. The hydraulic arrangement seemed to present advantages which were apart from its use as a propeller; but wherever it was necessary to put a scoop into the bottom of a ship it would be found that there is a strong objection to it, because the scoop would be the first thing to encounter the ground on getting into shallow water. It would not, therefore, be liked for that purpose, however applicable it might be to a torpedo boat. He had lost all faith in coefficients. He had lately seen a statement that the large and fast steamer, the Phæton, sister ship to the Iris, had had about 600,000 lbs. weight of engine taken out, the engine-power being reduced from 7,000 H. P. to 5,000, and had been driven faster (more than 18 knots, with the same displacement and the same shape), and he confessed that coefficients would require a little more explanation before he could recognize their applicability to the sub-

ject. Engineers were apt to make a formula to fit certain cases, and to lay it down that those cases were the rule, and that all abnormal cases, as they were called, were the exception. It was strange that with the addition of 600,000 lbs. to the armor-plated deck, one vessel with 5,000 H. P. went 18.3 knots an hour, while the other vessel with 7,000 H. P. went 18 knots. An experiment mentioned to him some years ago by Mr. Gwynne, of Hammersmith, gave the following results: Diameter of disk, 30½ inches; orifice, 8 inches; revolutions, 800 per minute; indicated H. P., 100; head attained 90 feet, with a discharge of 800 gallons per minute; thus, there was a pressure of 45 lbs. per square inch. The effective thrust was 2 tons. Of course, to the 45 lbs. per square inch would have to be added the 800 gallons per minute discharged at the top of the 90 feet. There was an important anomaly in all the records of the Waterwitch. Engineers would readily admit that there was no departing from the law of action and reaction being equal and contrary, and when any one told him that a pump, whose peripheral velocity was 29.3 feet per second, gave a velocity of discharge of 29 feet per second, he thought some other explanation should be sought than that which was found in the pump itself. Unless the velocity of the feed was there utilized, he did not see how such an effect could be produced. If it were produced, the pump would be undoubtedly an absolutely efficient machine. It might be expected that as the disk-pumps were enlarged there would be an increase of efficiency. All experience pointed to that. While in the smaller pumps the velocity of the issuing water was not more than about 50 per cent. of the peripheral velocity, in the larger pumps 60, and even 70, per cent. had been obtained. Distrusting formulas, he took the percentage by calculating the revolutions into the circumference, finding the peripheral velocity of the disk from that, and getting the discharge as a percentage of that peripheral velocity. When 37.2 feet per second had been obtained as the velocity of discharge, and the velocity of feed was 20 feet per second, he presumed that only 17.2 feet per second had been communicated by the engine to the water. If that was the case, might not the anomaly in the Waterwitch be attributed to the velocity of feed from those inlets? A set of mouths, immediately under the pump, scooped downwards, as was the case in the later trials of the Waterwitch would give a certain velocity of feed into the pump, and that might be added to the peripheral velocity, so explaining what otherwise seemed to be utterly inexplicable? He had found that various sources were accessible for an account of the revolutions of the Waterwitch engines, and while he had found them sometimes stated at forty-two, they occasionally descended to forty per minute. Engineers, he thought, would be thoroughly aware of the vice of taking revolutions either through another observer or at a different time from that at which the indicator diagrams were accorded. Any possible H. P. might be indicated in such a way, and give rise to a very false basis of coefficients. It appeared that in the Thornycroft propeller a velocity of 56 feet per second had been obtained—he presumed a peripheral velocity—and the velocity of discharge was 37.25 feet. That velocity was much more important than quantity of discharge. The volume did not matter in the least in regard to the velocity obtained. The whole power of the engines might be thrown away in lifting a mass of water, but if the necessary velocity to produce the speed sought was not communicated to the water, most engineers would say it was vain to seek it at all. It would be interesting to try an experiment first by towing the boat with her inlet so arranged and her scoop so arranged at the velocity which she had obtained with the engines at rest. It would then be seen how much the velocity of the inlet-water was diminished by passing through the pump at rest. Next, to tow the boat with such revolutions of her engines as would give a peripheral velocity equal to the speed of the boat. And, lastly, at the calculated velocity of feed. Those three experiments would bring out exactly the efficiency produced by the feed as distinguished from that produced by the pump. Now, supposing that the revolutions of the screw-engines had not been changed, but that the diameter of the nozzles had been diminished from 9 inches to 6 inches, and the disk increased to 2 feet 9 inches, what might be

expected? He thought it would be found, taking the basis obtained by the author's experiments of 56 feet peripheral velocity, and 37 feet velocity of discharge, that 60 ft. per second velocity of discharge should be obtained with 91 ft. per second velocity of periphery. That would correspond, provided the water thrown was sufficient to bring out a proportionate pressure. He was not aware whether the pressure per square foot of midship section to drive the boat 17.3 knots per hour had been calculated, but, whatever it was, it could no doubt be made up. The velocity of 91 ft. per second peripheral, and 60 ft. discharge, would give about 17.7 knots per hour. He was sure that would be impracticable with a less speed of discharge water. If the number of revolutions per minute were diminished, the speed of the boat could be arranged without any possibility of change. Every experiment he had made brought out the fact that the speed of revolutions was indissolubly connected with the speed of the boat. If, therefore, the principles underlying the subject had been thoroughly understood, the first question asked would have been, "What speed is it wished to get from the boat?" That would govern the revolutions, or the revolutions multiplied into the periphery of the disk. The disk might be made a little larger and then the revolutions might be diminished, or the revolutions might be increased and the disk diminished. But, as Mr. Gwynne's experiments showed eight hundred revolutions producing a thoroughly good effect, with one 8-inch issue, there need be no fear. He had himself driven a small 1-foot pump at one thousand revolutions, and on the basis to which he had referred he had obtained exactly the calculated speed of the boat. The pressure per square foot midship section was, as nearly as he could make out, at the velocity and size of nozzle stated, 120 lbs. He presumed that that would be scarcely sufficient for a speed of 17.7 knots per hour, but it could be easily increased. In regard to the possibility of pumping the ship out with the full power of the engines, if any issue was opened from a drowned compartment (where the water must be on a higher level) to the turbine, he failed to see why the turbine should choose between the bilge-water and the other water. He thought it would certainly take a larger quantity from the easier path of the inlet, but that it would take a portion from the bilge that would probably be sufficient to keep the boat from sinking, or result in so far prolonging its floating above water as to save human life. That result would be sufficient to justify any trials. It could scarcely be supposed that in the event of another war there would not be a lesson to learn as to the effects of rams and torpedoes. Mr. Thornycroft had done so much and so well in providing the means of destroying the British and other navies that it was only fair to ask him to throw a little of his energy, skill, and genius into the question of how to save human life and the expenditure of ships.

Sir Edward J. Reed, K. C. B., M. P., thought the paper was a very important contribution to the knowledge of the subject of hydraulic propulsion. The last bearing on the subject was the comparison, referred to by Mr. Ruthven and other speakers, between the Viper and the Waterwitch. Admiral Selwyn had expressed his distrust of coefficients and of formulas, and Sir Edward Reed supposed that all engineers distrusted them when they only took account of a fractional part of the circumstances they wished to consider. There were very peculiar circumstances in the case of the Viper and Waterwitch trials. In the first place he might say, as the designer of the boat, that the Viper was about the very worst screw-propelled vessel that had ever been built, and that for a very good and sufficient reason. She had to be designed to pass through certain locks; her length and her breadth, therefore, were arbitrarily determined, and her depth, also, had been similarly determined. Having thus a parallelopipedon which was to embrace the Viper, there had to be carried by it certain armor and guns, and particularly as much as possible of the armor. The speed was of very minor importance, and it had been deliberately subordinated to the armor-carrying and gun-carrying power. It would therefore be seen that the Viper and Vixen were rectangular boxes, slightly improved, with small regard to propulsive advantages, and he thought they were about as bad vessels as could

have been designed. Admiral Sir George Elliot, impressed, like Admiral Selwyn and many others, with the great advantages of hydraulic propulsion to Her Majesty's ships at sea, put a strong pressure upon the Admiralty to get the hydraulic propeller tried, and holding at the time a high official position, and being an officer of great experience at sea, and of much deserved influence, he succeeded in inducing the authorities to try a hydraulic propeller in a vessel of the class referred to, or a vessel of about the same dimensions. There were reasons, however, tending to show (the figures given by the author did not fully demonstrate it, but he had no doubt Mr. Ruthven would readily concur) that the form of the Waterwitch was superior to that of the Viper and the Vixen, because, when she was designed, some limitations imposed upon the other vessels had been removed. A considerable improvement in form was realized, and the trials were made, and, as far as his memory went, the figures given by the author fairly represented the state of the case.

Taking Mr. Ruthven's figures, it would be seen that the bad screw-propelled vessel was a little better than the hydraulic-propelled vessel, even at the low speed of 9½ knots per hour. The trial certainly made a good show for the hydraulic propeller by comparison with a bad screw-vessel—a better result than had been expected by many persons; but what would happen when higher speeds were attempted? The author's paper contained a case in point. The Government had sought to give effect to the recommendation of some of the members of Lord Dufferin's committee, or of the committee itself. They had gone to a gentleman who had distinguished himself in the design and production of vessels of the torpedo class, Mr. Thornycroft, to whose genius the introduction of these light and extremely fast vessels was due, and they had asked him to do the best he could in establishing a comparison between the hydraulic and the screw propeller designed for high speed. The result was, that with a given H. P. in two vessels, made as nearly alike as the circumstances would admit, the hydraulic vessel steamed at a little over 12½ knots per hour, and the screw-propelled vessel at a little over 17¼ knots. Admiral Selwyn had referred to the statement in the paper that the jet as a propeller might be taken as a little better than a screw, but he had forgotten to complete the sentence: "But the loss in the pump is a dead loss, and represents about half of the power." He did not know what the experience of others might be, but he could say for himself that, if he had to devise a fast or any other steam vessel, to lose half the power in order to secure an efficient propeller, would be a very sad imposition to have placed upon him. He was unable, as he always had been, to anticipate any satisfactory result from the hydraulic system of propulsion as applied by means of internal pumps in the ship. He sympathized with the objects that Admiral Selwyn and other naval officers had in view, and he should be delighted to see growing out of the discussion some relief from the loss which the system always involved when so applied. He confessed that his sympathies went very strongly in favor of the apparatus described by the author after the reading of his paper, viz., the application outside the vessel of an arrangement by which the good effects of hydraulic propulsion might be obtained in a large degree without its serious disadvantages. With regard to Mr. Ruthven's reference to the mode of conducting the trials, he thought he ought in fairness to say that everything had been done at the time that could, in reason, be done to satisfy Admiral Elliot, who was known to be more or less in communication with Mr. Ruthven. He had also to state that he believed that the consideration of the subject had been brought to an end because the discussions between Admiral Elliot and the Admiralty had passed away from the efficiency of the jet system to questions about the indications of power in the engines of the Waterwitch, and other collateral matters, so that the essential part of the subject had in some way fallen out of discussion. He was sure it would be the desire of the council and of the members that every possible opportunity should be given to gentlemen capable of discussing the subject, to direct the minds of the members to some methods, if methods there were, by which to get rid of the enormous loss sustained with-

in a ship by the application of the hydraulic system for its propulsion.

Admiral Sir George Elliot, K. C. B., said he would not venture to enter the domain of science; all that he knew of hydraulics he had learned from personal observation of the practical results which had been hitherto obtained. He was greatly pleased when he heard that the subject was to be discussed by the Institution, on account of the great advantages to navigation which would accrue even from a partial application of hydraulic propulsion to ships. He did not think that hydraulic propulsion had ever yet had fair play; it had never yet been tried on its merits. Certain conditions had been attached to the experiments which he thought had marred the best efforts of engineers. The Nautilus had not been designed with the sole view of developing hydraulic motive power in a ship; the owner had a commercial object in view. He looked to a commercial enterprise, inasmuch as the vessel was designed to run up the Thames to Richmond; consequently, with a light draught, and having to carry a large number of passengers, she had full lines; still the speed realized by that vessel was 9½ knots per hour. He thought the author had made a mistake when he said 8.32 knots. She was raced on the river, against a vessel called the Volunteer, down to Gravesend, and she distanced her. The Volunteer was a vessel of 24 H. P., whilst the Nautilus was 20 H. P. She afterwards raced constantly with the Citizen boats, and to his knowlege she beat them. Then a challenge was held out to any paddle-boat on the river, but it was not accepted, although an offer was made to pay the expenses. No one could say that the Waterwitch was a good specimen of naval architecture in which to try an experiment. The constructors had a very difficult task in making armor-clad gunboats, and the designer looked upon them as very bad specimens of naval architecture. However, the Waterwitch, in her first trial at the Maplin Sands realized a speed of 10 knots per hour.

At that time in a contest between the Viper and the Vixen, her two sister vessels, the latter was the slower of the two, therefore the trials afterwards took place between the Viper and the Waterwitch; the former vessel at the Maplin Sands obtained the same speed as the Waterwitch. They were afterwards taken to Portsmouth where the trials went on. The builder's trim of the Viper was 1 foot at the stern, at which she was run at the Maplin Sands; but at Portsmouth she was brought down to 2 feet at the stern, and the bow lines were thus lengthened, whilst the Waterwitch was compelled to remain on an even keel. The trials gave the advantage to the Viper of ¼ of a knot speed. In the Waterwitch the water came in to the turbine from forward through a canal, with holes perforated in the bottom. The turbine was raised unnecessarily high, and the discharge pipes were raised still higher, in order to test the differences between discharging the water above water when she was at her light trim, and in the water when she came down to her load draught. He considered both these features to have been adverse to the success of the hydraulic method of propulsion. Those vessels went to sea, but they were found to roll so heavily as to be unseaworthy, and they were afterwards laid up and lightened and taken out to Bermuda; the Waterwitch was laid up in ordinary. Some years afterwards, when he was appointed Dockyard Superintendent at Portsmouth, the Admiralty allowed him to have the Waterwitch to make some experiments. He had an idea that leading the water in through the bottom of the vessel in a canal, by which a large quantity of water was carried about, necessitating increased displacement, was a mistake, and he was determined to ascertain how far it would be advantageous to bring the water in from forward or from directly underneath the turbine. The trials were carried out very carefully by the dockyard officers. He merely asked them to close the canal and to open underneath the turbine a hole and to insert a tube, and let the water enter directly underneath the turbine. The vessel was taken into dock and cleaned, put to a certain trim, run at the mile, and her speed ascertained. She was then docked. The alteration was made; she was again cleaned, put to the same trim, taken to the mile, and the result was ½ knot additional speed per hour. He then thought that the turbine to be at its best should be brought as low in the vessel as possible, the water

admitted from underneath, and the discharge-pipes horizontal. In the Waterwitch they were raised, as he had said, which was certainly an objection. He still believed that, in order to try the hydraulic-propeller at its best and give it fair play, a vessel should be designed for the purpose, a double-ended vessel with a rudder at both ends, on an even keel, and the hydraulic-propeller as low as possible, in point of fact to work in the water. He had reasons for believing that bringing the water up to the turbine from directly underneath was an advantage. His idea was that directly the turbine was turned, a hole could not be made in the water, and a column of water at a certain speed was immediately set in motion, which was greater than the speed at which the ship was going through the water; consequently the greater velocity overcame the lesser velocity due to the momentum of the vessel; in fact there was no loss of power. If the turbine was very low down the bottom of the turbine might almost be brought to the sea itself. Having a rudder at both ends the vessel would go equally fast both ways; in fact, in the trials in the Waterwitch, although the water was brought in forward it did not make much difference in her speed one way or the other. That told very much against the attempts that had been made to draw the water in from forward by pipes from the bows. He could understand the object in view; and theoretically he could see that it appeared to be a wise one; but as far as that trial in the Waterwitch went, it proved not to be so. Then he made an experiment to ascertain what the size of the discharge pipes should be. The Admiralty were again good enough to carry out his experiment, by putting a flap on a hinge inside the nozzle, which could be pushed out, so as to close the aperture or leave it wide open. The speed of the water at the discharge was measured, and it was found when the nozzle was half closed the water was going out at double the speed; therefore the same quantity of water was discharged as when it was wide open—the speed of the vessel in both instances being the same. That showed that the nozzles of the Waterwitch were unnecessarily large, because he believed that the best result was obtained when the pipes were completely filled, but trials were made at different revolutions of the turbine, and it did not appear to signify much. He did not agree that the turbine would not work unless it was full. The author had described all the advantages (and they were very great) of the turbine as a propeller for ocean purposes, but he had not specially named one benefit which he thought the most important. If a vessel could be stopped, without any reference to the engine-room, in her own length, and if two vessels meeting could do the same, he believed there would be very few collisions. Another feature was the advantage that it afforded in foul weather. The accidents in screw-boats that then occurred were generally caused from the racing of the screw; but the greatest amount of motion a ship could be put into would not in the slightest degree bring a greater strain upon the engines of the turbine. After a considerable time, the subject was again mooted at the Admiralty, and then came the trial of the Thornycroft torpedo-boat, respecting which he was greatly pleased to think that the experimental trial had been placed in the hands of that firm, who, he had no doubt, had taken pains to make it a success. But as a competitive trial it was inadequate. The author had stated that the object of the Admiralty was to have vessels of very light draught of water in waters so shallow as scarcely to afford sufficient immersion even for twin screws. In a fair trial both vessels should have been put to the same test, which was the draught of water. But in the case of the turbine the draught of water was 2 feet 6 inches, while that of her opponent was 3 feet 8½ inches; and in the screw-boat the shaft was on the keel, and nearly half the blades revolved below the keel, giving a large diameter to the screw, added to the extra draught of water. In this competitive trial the screw torpedo-boat would require a depth of 6 feet of water to work the screw, whilst the turbine boat would only need a depth of 2 feet 6 inches to move about in. That was unfair to the turbine which had not those advantages. The two trial vessels should have had the same draught of water, similar displacement, and similar power.

If the draught had been 2 feet 6

inches, and the screw had been limited to that diameter, he thought that a very different result would have been obtained. He should be much pleased if Messrs. Thornycroft and Co. would build a boat exactly as he had described, with her turbine low down, with the entrance of water from below, with the discharge-pipes horizontal ejecting the water under water, and with the valve like that in the Waterwitch, and then see what speed they could get out of the screw with a diameter of 2 feet 6 inches, with the same power. Then again one boat was brought by the stern, and the other was on an even keel; but both boats ought to be confined to a given draught of water. He would give the turbine boat rather a full body, so as to lower the turbine and get as much benefit from the full diameter as possible. In the vessel in question, the diameter of the turbine was small, and the discharge-pipes were long. It appeared to him that the importance of centrifugal force had been lost sight of, and this he had always understood to be an advantage in the turbine, by not having a large diameter so as to discharge the water at a rapid rate through the pipe, and reduce the revolutions of the turbine. Another advantage of admitting the water from below was that it was not necessary to carry the same volume of water in the canal as had to be carried in the Waterwitch; it was only necessary to carry the water which the turbine itself contained. When he spoke of the partial success of the turbine, he referred to its use in sea-going vessels as an auxiliary power. With power sufficient to give a speed of 8 knots per hour, to get across calms and in and out of harbor, an enormous advantage would be gained. Unfortunately the hydraulic-propeller had not had many friends. Whenever he had approached the ship-builder on the subject he had been told that it would cost a great deal of money being a novelty, and that at once had put an end to the idea. On one occasion at Portsmouth a lady, in delicate health and unable to stand the vibration of a screw, was towed about in her yacht by a tug. He endeavored to get a turbine put into a vessel that the lady was having built, but it was stated that it would have added £6,000 to the cost. He should be glad to see hydraulic propulsion tried still further, and felt sure that better results would be obtained if it were tried fairly on its merits.

Admiral Ryder observed that he had paid some attention to the subject from the commencement. He had taken a run in the Nautilus, and two or three trips in the Waterwitch. A few days ago he went out from Portsmouth to Spithead in Torpedo Boat No. 87, which was irreverently called The Squirt. It was a moderately fine day. The vessel was a remarkably good shape forward for riding over the sea, but the two tubes for ejecting the torpedoes were unfortunately conduits for seas that came over forward and which dashed through the tubes against any one on deck with great force. The tubes should have wooden plugs. When going at 12 knots an hour, he said to the lieutenant in charge, "I think you may ease her," and his reply was, "We must either stop or go full speed." If that was the fact, it was certainly an unfortunate position for one of Her Majesty's vessels to be in. The sensation was peculiar when the boat stopped short. She stopped very dead, which, of course, was what was wanted in the case of a torpedo-boat. But he could not make out why the ejection orifices were above water. The noise was like soda-water bottles going off continuously. The tremendous noise would, he should think, on a calm day inform an enemy 10 miles off of the approach of the boat. He could not believe that it was in the specification that the orifices should be above water. Perhaps there was some good reason for it, but from his point of view as a sailor this feature was a most objectionable one. The nuisance of the noise was intolerable, and he thought that any Admiral would send the boat home by the first ship; he hoped that in future designs the orifices would be under water. One of the great advantages of that class of torpedo-boat was the freedom from danger of fouling. Seafaring men and engineers could easily picture what would happen on going into a harbor protected by nets with the ordinary form of torpedo-boat having one or two enormous screws; these would be sure to come in contact with any obstruction of that kind. It was all very well for the open sea, but he did not believe that such a boat would

et inside an enemy's harbor at all. The turbine principle was a capital one for that purpose, because there could be no fouling by nets or anything of the kind. Another valuable feature was the power of changing the course to the opposite direction suddenly. A torpedo-boat fitted with any system of screws could work end on, bow foremost, but could do nothing against the enemy stern foremost; whereas if the turbine principle were further developed, and applied to larger vessels, an enormous advantage would be gained; indeed, the utility of the ship might be almost doubled. After having gone through the enemy's fleet, and struck or missed a ship, a turbine vessel might at once reverse her engines and retrace her course, without exposing her side, as the screw-vessel would do when turning. Of course the great question was whether the numerous advantages named by the author, not omitting that invaluable feature that the turbine propeller could not be "pitched" out of the water, as the screw frequently was, were worth paying for, and to what amount. They might be paid for in reduced speed.

Would it be worth while sacrificing 2 knots, or how many knots, an hour to obtain the advantages that had been described? A committee of naval officers would soon come to a determination on that point. If the authorities would not pay in speed, would they pay in money? It might be necessary to get a larger or a narrower boat, or to give up some other feature to which importance was attached. He agreed with Admiral Sir George Elliot and Admiral Selwyn that the turbine question was not yet settled. The system had been tried by different nations. Swedish naval officers attached great importance to it. When he was commander-in-chief at Portsmouth, whenever a Swedish man-of-war came in, he always asked the officers how the turbine system was getting on. Their reply was that it was so unpopular with the Construction Department that they were not at all sanguine that it would be favorably received. At length, however, a turbine vessel was tried, and, on enquiring the result, the reply he received was, "They have managed to make it fail." A round sum in thousands of pounds should be offered for the best design of a turbine torpedo boat, capable of a speed of 20 knots per hour, and with orifices under water.

Mr. J. Wright, C. B., Engineer-in-Chief of the Navy, said that the subject had been so fully and fairly discussed in the paper that he had very few remarks to make; but there was one point which had not been much referred to during the discussion, namely, the great advantage supposed to accrue to a vessel with a hydraulic propeller in turning power. It had been tried in the case of the Waterwitch and the Viper, and the advantage was certainly very much in favor of the Viper. It took the Waterwitch twice the time to turn a circle that the Viper took when one screw was worked ahead and the other astern. Another point of importance was the great advantage claimed for the hydraulic propeller in clearing the ship of water in the event of her getting a hole in the bottom. The author had referred to the practical difficulties which would have to be overcome in opening and closing large sluice valves in order to carry out such an arrangement. From some of the preceding remarks it might be supposed that the naval architects of the present day had given no attention to the important matter of saving life on board ship, in the event of a large hole being made in the bottom; but any one acquainted with modern ships of war would know that an extensive subdivision was carried out for that very purpose, so that if two, or even more, compartments were penetrated, a large power of flotation would still be left. If a hydraulic propeller were fitted for ejecting water from the ship, it was just possible that if a hole were made by a torpedo in the compartment containing the propeller, the propeller would be disabled. If, again, a hole were made in another part of the ship, there would be a difficulty in getting the water under the propeller in anything like sufficient quantity to keep it charged. No doubt Admiral Sir George Elliot had made an improvement by closing up the holes in the Waterwitch's channel forward, and opening a direct hole under the bottom. The coefficients of the performances (though one gentleman had declared his disbelief in coefficients) showed an improvement of something like 15 per cent. But, with regard to other experiments

made by Admiral Sir George Elliot, in curtailing the size of the discharge nozzle, there was a decided falling off to the extent of over 7 per cent. There was another point with reference to the Viper, namely, that the screw propeller acted on ten times the amount of water that passed through the turbine. If any one would examine the theory of the subject, he would find that that was a very important matter in regard to the superior efficiency of the screw. Comparing the two torpedo boats in the same manner, it would be found that the screw boat acted upon five times the amount of water that the turbine had acted upon.

Mr. R. Sennett observed that there could be no doubt that the torpedo boat referred to was the outcome of engineering skill of the highest order, and of workmanship of the greatest perfection; and with what result? Practically this, that the speed attained was no greater than could be obtained from a screw-boat of equal size working at about one-half the power. The causes of this defective performance were not difficult to determine, and were, in the main, inherent to the system. All propellers worked on the same principle, namely, the projection of a volume of water sternward, and the thrust of the propeller was measured by the momentum or change of momentum produced. The more directly sternward the water was projected the greater the efficiency, all transverse motion representing wasted energy. In the jet every particle was sent directly astern, and therefore, theoretically, neglecting friction, the jet was the most perfect of propellers. It was from practical difficulties that the pump failed as a propeller; and though engineers were loth to apply the word impossible, yet in this case many of the obstacles were apparently insurmountable, and in his opinion the pump was never likely to become a general and efficient propelling agent, though it might, perhaps, be useful in some special cases. This arose mainly, as had just been pointed out, from the large volume of water that had to be dealt with, and the large orifices that would be necessary in the ship's sides to obtain high efficiency of propulsion. The thrust of a propeller depended on the mass of water operated on, and consequently, other things being equal, the propeller that could operate on the greatest volume of water would be the most efficient. Perhaps the custom of speaking of this propeller as a jet propeller might unintentionally convey the idea that it was different in principle from other propellers. This, however, was not so. The jet was no more the propeller in this case than was the race of the screw or paddle in ordinary ships. As a matter of fact, the pump was the propeller, for it was the agent that drove the water astern from the momentum of which propulsion resulted, just as the screw and the paddle were the machines for driving the water astern in ordinary cases. In the hydraulic boat the propeller was placed inside, in the paddle and screw outside; the action in each case was the same, only the machine was different. Until pumps were fitted that could cope with as large volumes of water as the feathering wheel, and at about the same speed, the hydraulic propeller would not be able to approach in practical efficiency either the screw or the paddle. irrespective of the greater losses that might be expected from the friction of the water in the passages. If the pump were made to satisfy these conditions, the large orifices necessary would, in most cases, be inadmissible, whether above or below the water. Then, also, the question would arise, would it be desirable to place such a propeller in what would practically be a trunk in the ship, occupying valuable space, and involving the carrying of a considerable weight of water? Or, would it not be preferable to place the propeller outside the ship, and make proper provision for a sufficient flow of water to it, without compelling the water to pass through and be carried by the ship? He had little doubt as to the answer that would be given by the vast majority of naval architects to this practical question.

Mr. John I. Thornycroft said that Captain Heathorn appeared to convey the idea that the friction of the external surface of the stream of issuing water had some advantageous influence in propelling a vessel, and he applied his remark to an experiment which he had made where an induced current of water was used to augment the propelling effect. He desired to go into some detail in order to explain the matter. A stream of water issuing from a vessel had a re-

action depending simply on the weight discharged in a given time and the velocity of discharge; the effect was unaltered by the form of aperture at which the stream found exit. The stream being submerged made no change in the reaction of the initial stream, but it then was in a position to induce other streams which might act favorably or unfavorably in propelling—favorably when the pressure was reduced on a surface attached to the vessel which had a motion towards the loss of pressure, and the contrary, if motion of the surface was in the opposite direction. One way of taking advantage of this principle was to surround the stream of water from an ordinary screw propeller by a conical tube of such dimensions as to allow an annular stream to pass through it around the stream from the propeller. When this was so placed that the initial stream entered at the larger end of a portion of a cone, and by the friction of its external surface and intermingling with the surrounding water, induced a stream through the coned tube, the pressure within this tube would be reduced, and the forward component of the difference of internal and external pressure would be available for propulsion. But, in order to estimate the useful effect, the friction of the surface of the coned tube must be taken from the forward component of the pressure already alluded to. Experimentally he found that if a vessel was fitted to utilize the currents so generated, although it would be able to exert a greater pull at a fixed object, in still water this advantage would diminish as speed was attained, and it appeared that at a useful velocity of vessel the apparatus was a failure; but he thought the result obtained by Captain Heathorn must be explained in another way. He thought that Captain Heathorn had fallen into an error. He had described an experiment that he had made, and he understood him to say that from water issuing from a pipe he got a certain reaction, and by contracting the pipe so that less water came out he got a greater reaction. He thought it was evident that in the first case most of the work was done by the friction of the water in the pipe, and in the second case, although less water came out, it came out at a higher velocity, and so the result was obtained. Captain Heathorn's allusion, therefore, to the apparatus to which he had referred was not correct. Admiral Selwyn had alluded to a pump, which he said gave a certain duty, and he spoke of 100 H. P. working a disk of 30½ inches in diameter at eight hundred revolutions, and raising 800 gallons a minute 90 feet. On looking at it, Mr. Thornycroft expected to find a good result; but he had computed the efficiency to be only a little above 0.22. Admiral Selwyn further stated that there was a pressure of 45 lbs. per square inch, and that to that would have to be added 800 gallons per minute discharged on the top of the 90 feet. At first he was puzzled to know how that was to be added; but in taking into consideration the 8-inch pipe, which he described, he found that the water flowed through it at 6 feet per second, and what was really to be added to the 90 feet was, therefore, the head due to the velocity of 6 feet per second. On working that out he found that it came to about ½ foot, and instead of lifting water 90 feet it was lifted 90½ feet. He did not know what the percentage was, but it was perhaps 22 or 22½, so that he was afraid that the pump mentioned by Admiral Selwyn did not compare favorably with his own. The discharge of the pump described by Admiral Selwyn was immensely less than the discharge from the pump of the hydraulic boat, although the disk was the same size and the revolutions were nearly double. Admiral Selwyn had stated that in what he considered a perfect pump the water discharged had the same velocity as the periphery of the disk. That, he thought, was entirely illusory. In one case it might be nearly correct. If only a little water was allowed to flow through a large pump, so that the water was a long while in the disk, no matter of what shape the vanes were, the water in the disk attained practically the same velocity as the disk of the pump. He believed that when the flow was limited in that way, all pumps gave about the same lift with the same speed. He had exhibited some diagrams in which the form of the blade had been considered. In the case of the hydraulic boat that he had constructed, the discharge was large, and the motion of the water through the disk was rapid; therefore, the effect of the blade form was very

manifest. In considering the effect of the shape of the blade, it was necessary to take into account the speed of the water from the center to the edge of the disk. The result of the diagram was to show that, taking into account the form of the blade, the water flowed through the pump at just the velocity it ought to do. In the pump of the Waterwitch, the sectional area of the channel was so large that the water made a turn, and half another turn, around the disk before leaving the pump disk.

The result came out that the velocity of the periphery was 29.6 feet per second, and the velocity as measured in an equal interval of time was 29 feet. It was exactly 29 in the author's figures, which was perhaps too close a coincidence. In the case of the Swedish pump the result was not so coincident, the velocity being only 21 out of 23 feet per second. Admiral Selwyn, therefore, was incorrect in saying that a perfect pump was a pump in which the velocity of discharge equaled the velocity of the periphery. He might say further, that if instead of using the radial blade a scoop blade throwing the water forwards were used, under favorable conditions it would possibly make the discharge equal to double the velocity of the periphery, it being assumed that friction might be neglected. It would, therefore, be more than a perfect pump, because by Admiral Selwyn's estimate it would be a pump with an efficiency of 2. Captain Heathorn had stated that the channel screw was invented by the late Mr. Robert Griffiths. He believed that Mr. Griffiths did put a screw in a tube; but the idea of putting a screw in a tube with vanes to alter the direction of the water from the spiral to a direct run out was due to Mr. Arthur Rigg. He put curved vanes behind the screw, and so got some advantage; but he did not contract the channel as the velocity was increased, and in that way secure the full benefit from the arrangement. Admiral Selwyn had calculated the reaction of Mr. Gwynne's pump at about 2 tons. That was surely too high, because he found that a cylindrical column of water 90 feet high and 8 inches in diameter would weigh about 1 ton. In a jet-propeller double that, or about 2 tons, would be obtained if it issued at its own speed, but in the case in question it only issued at 6 feet per second. If the water would issue as slowly as that, and give all the reaction, it would be correct; but, as it would escape at a much greater speed than that, the water would not supply the nozzle. Admiral Sir George Elliot had referred to the Waterwitch, and to the way in which the water was led to the propeller. He believed his statement was correct, that the arrangement for leading the water to the pump through a canal was inefficient. Where the water entered through parallel plates the velocity was not converted into pressure. In order to convert the speed of a stream into pressure it was necessary that it should enter the small end of a tapering canal. He was not surprised that the channel was found from its surface friction to retard the effect of the propeller. Admiral Sir George Elliot had stated that the discharge seemed to be independent of the size of the nozzle. But Mr. Thornycroft thought there must have been some inaccuracy in the measurement of the speed with which the water had been ejected. With reference to the question of stopping a ship in its own length, it should be remembered that was quite a different thing from stopping a boat in one length. It appeared to be intended to put very large powers in ships; but in the case of a merchant steamer steaming at 14 knots per hour it was admitted that in starting the vessel the propeller was acting efficiently. A large steamer that would go at 14 knots per hour took a long time to attain full velocity, and if the propeller acted efficiently he supposed it would take the same time to stop. Admiral Sir George Elliot considered that the trial to which reference had been made had not been a fair one, and that the boats could not be fairly compared. Nevertheless, the best had been done, and even looking at it at the present time, after seeing where a slight failure might have happened, he did not know that if the work had to be done again it could be done much better. He thought it must be admitted that the discharge being above water-mark was a very great inconvenience. Whether the water could be discharged without loss below water-mark he was not sure. It would involve carrying a great deal

more water within the boat, and it would have to be taken obliquely out of the skin. If the present nozzles were projected into the water so that they were forced through the sea he was sure the loss of speed would be very great.

Mr. G. B. Rennie remarked that the extraordinary speed of 17½ knots per hour in the case of the last torpedo-boat had been arrived at after a great many trials; the speed of the first boats of the same size had been only about 14 knots, so that the comparison was hardly a fair one. In trying various forms of pumps several years ago, in experiments for some large pumping machinery,* he found that the efficiency of the "Swedish" pump far exceeded that of the Thornycroft, which was very much like the pump described by Professor Rankine.† The total efficiency of the pump and engine varied from 0.30 to 0.50. He did not think it got so low as 0.25, as in the cases alluded to in the paper. The way in which the calculation was made was this: the dock was full, and it was pumped out; the level was taken every ten minutes, and the H.P., and the total quantity delivered was divided by the H.P., the result being from 0.30 to 0.50 of efficiency. In the case of a propelling ship he could not see why that should be very much varied if the proportions were suitable. It had been stated that one of the problems to be solved in the hydraulic-propeller was to get sufficient water to propel the vessel. In the case of the Viper it appeared that about five times the volume of water was discharged or pushed aft as compared with that in the Waterwitch. That had always seemed to him a great difficulty with the hydraulic system. About three years ago his firm was asked to supply a floating-dock at a certain distant port. There was a great difficulty in re-erecting the dock abroad, and towing it was considered to be an impossibility. The question was how it was to be propelled. A screw-propeller seemed to be inadmissible from the draught of water, and from the inconvenience of a screw-propeller when docking a vessel. They therefore turned their attention to the question of hydraulic propulsion, taking the Waterwitch's experiments as a basis. He had a model showing how hydraulic propulsion might in such a case be advantageously employed. There were a great many compartments in the dock, and a great many pumps for emptying them. It was proposed to increase the pumps and power of the engines, and to make six apertures on each side adapted for propulsion. By that means a fair proportion of propelling area was obtained by the water discharged. The total area of all the openings was about the same as the combined area of one or two or three screw-propellers which would be necessary to propel a vessel, having a given section and capable of carrying a given tonnage, through the water. He thought that in the case of vessels of very light draught of water, and short broad vessels carrying heavy weights, the application of a screw-propeller was almost an impossibility. But while in the one case it might be impossible to apply the screw-propeller, in the other case, namely by hydraulic arrangements, it was possible to propel a vessel at a certain speed. It would be interesting if the author would explain somewhat more fully the instrument to which he had referred for taking the pressure of the discharge at the nozzles.

Mr. Edwin N. Henwood considered the hydraulic-propeller so superior to the screw, that it would be unwise to think any money wasted that would enable its advantages to be brought out, and its efficiency to be developed. If a like amount of money that had been expended on the screw-propeller were to be expended on the turbine, he maintained that the success of the latter would be secured. With reference to the comparison of the Viper and the Waterwitch, the former was a very objectionable structure; the Waterwitch was nearly as bad, but even in the latter the turbine did not have its advantages developed; it was fitted horizontally, and hence one of the streams from the discharge pipes had to be turned by a curved pipe into its proper path. The mode adopted of measuring the velocity of discharge was thoroughly unsatisfactory, as had been remarked in the paper, since it was well known that a patent log must be carefully kept out of disturbed water in

* Transactions of the Institution of Naval Architects, vol. xxiv., 1883, p. 126.
† "The Steam-Engine," p. 190.

order to get a true result. Nor did he admit that the arrangement described of a pressure-plate $1\frac{5}{16}$ inch square, supported by a more or less knife-edged lever, could be considered entirely trustworthy. With regard to the efficiency of a pump or other machine, it was needless to introduce midship section or displacement coefficient into the equation. When the designers fixed the diameter of the disk, and the number of revolutions of the engines, they had fixed the maximum velocity of discharge possible, and therefore the speed of the vessel; and no reduction of the nozzle, with the object of increasing the velocity of discharge, would materially influence the effective thrust. It appeared to him that the experiments were so devised—in error, no doubt—as to ensure no greater result than that obtained; and the paper really seemed to lead to a double dilemma. Either the Waterwitch pump was effective within $\frac{3}{10}$ of unity, or the feed was an important element in the total velocity of discharge. If the former proposition were true, then there was, under circumstances easy of repetition, no loss whatever in the pump, while in the screw there was always a large percentage of loss, without taking into account pitching, &c. If the latter were the correct explanation, not only was the result from the velocity of feed quite as great as in the Thornycroft, but the velocity of discharge was much better for the revolutions and the disk diameter. The revolutions in the Waterwitch were forty per minute, and in the Thornycroft four hundred and twenty-six; the diameter of the disk in the Waterwitch was 14 feet, and in the Thornycroft $2\frac{1}{2}$ feet. The velocity of the periphery in the Waterwitch was 29.3 feet per second, and in the Thornycroft 56 feet. The velocity of discharge in the Waterwitch was 29 feet, and in the Thornycroft 37.2 feet per second. If the velocity of discharge was more than had been observed, as seemed probable from her speed of about 10 knots per hour, those figures would have greater prominence. He was unable to find any reason for neglecting to drive the turbine at a higher velocity. The propeller in the Thornycroft boat was driven at six hundred and thirty-six revolutions per minute, and the revolutions of the turbine had been reduced to four hundred and twenty-six. He could not imagine why there should be any objection to driving the turbine at a higher velocity. He should like to ask if there was any objection, and, if so, what, to working the turbine in a vertical direction so as to discharge the water underneath the bottom of the vessel? Allusion had been made to the great difficulty in utilizing the turbine for keeping down any leak. He thought the difficulties were of a very slight character, and could easily be overcome. He wished also to ask whether the speed of the circulating water from the condenser was as great as that of the pump discharge, and whether the discharge was directed aft. He thought the superiority of the turbine-propeller over the screw-propeller in the case of fouling was very great. Everyone who had gone to sea would know what would happen if the propeller broke by meeting a floating substance. In a torpedo-boat propeller driven at six hundred revolutions a minute this would often happen. In the case of the turbine there was no such source of damage.

Captain J. D. Curtis, R. N., understood from Captain Crozier, who had charge of the experiments, that the performance of the Waterwitch was very good except when she got into a double-reef-topsail breeze. He presumed that this was on account of the pitching of the vessel. He looked upon a turbine as a kind of endless rope. Admiral Sir George Elliot had referred to the Viper being put 2 feet by the stern and going $\frac{1}{4}$ knot per hour faster. The Serapis, when put 2 feet to the stern, attained the same speed, 10 knots, with an expenditure of 50 tons of coal per twenty-four hours, as she did with 80 tons on an even keel. Reference had been made to the benefit to be derived from having an arrangement by which the screw should not foul. About two years ago the late Mr. Robert Griffiths took out a patent which he called the shield patent. This shield consisted of two plates, so arranged that they prevented the propeller acting on the deadwater, and increased the speed of a ship from 6 to 8 per cent. In addition, they prevented vibration, improved the steering, and reduced the racing when the ship was pitching. The following results had been obtained from a steam-launch to which it was applied, the own-

er, Mr. C. Bourrot, and Captain Curtis, R. N., being present at the trial:

	Without Shield.	With Shield.
Mean speed in knots per hour	6.417	6.636
Mean revolutions per minute	289	265
Mean pressure of steam in lbs. per square inch	45.1	42.5

showing a gain of 3½ per cent. in speed, with a saving of 15 per cent. of power.

The shield was inexpensive, the estimated cost for a propeller 12 feet in diameter being only £12, and it could be put on without docking the ship. Taking the cost of marine engines at £40 per nominal H.P., and allowing a similar sum for coal, also for the space occupied in the ship by the coal, and for repairs, &c., the increase of 6 per cent. in speed, to obtain which 19 per cent. more power would be required, was equivalent to an increase in the value of the ship of £1,520 for every 100 indicated H.P. of the engines. In this shield the upper segment went home to the stern of the vessel; the lower segment over one-half the length of the screw from aft forward. Four trials were made with a small vessel above Teddington Lock, two with the shield and two without, and Mr. Robert Griffiths stated that the shield effected a saving of 15 per cent. out of the 45 per cent. which Mr. Froude asserted to be lost by the screw. How was that? When the vessel went through the water, the water must follow the ship, or the ship could not make any progress. The use of the screw detracted from the power of the water to follow the ship, and that had not been sufficiently thought of and acted upon. The shield patent must not be confounded with the cylinder, as they acted differently, the former allowing the escape of water aft. Directly the screw went down, the water came through between the apertures of the shield, and the following water flowed in to occupy the place of the water displaced by the screw. The screw did not carry the load of water round, and did not curtail the amount of inflowing water. As the screw came up to the segment of the shield, it cut the volume of water carried around in the ordinary screw, and there was no vibration. Mr. Robert Griffiths used to say that he considered the screw a hydraulic pump, and that for efficiency it was placed too near the ship's run. By another patent he placed the propeller two-thirds of its diameter away from the foremost sternpost, and in this way allowed the following water to act upon the ship, and did not detract from the speed of the vessel. In conclusion, he might observe that aquatic birds struck out alternately with their feet when swimming, economizing power, the thrust not interfering with or obstructing the following water; and that he had heard a Yarmouth fisherman, when giving directions to the builder of his smack, say, "Give her a cod's head, and a duck's tail for a stern."

Mr. N. Barnaby, C. B., Director of Naval Construction, said the discussion on the paper was remarkable, for two or three reasons, one of which was that amongst those who took part in it was the gentleman who, forty-five years ago, had patented the mode of propulsion in question. Another was that for the last twenty years the Admiralty had been endeavoring to get some sort of success with the jet propeller, and that they had not been able to point to any success. A third reason was that he believed there did not exist at the present moment any vessel in the mercantile marine employed on a passage of any kind which was propelled in that manner. Accordingly he expected that when the paper was read he should have been called upon to explain how it came about that the Admiralty still considered it right to go on trying the jet propeller; but he presumed, after the speeches which had been made by the gentlemen who had been influential during the last twenty years in urging upon the Admiralty the various trials of that mode of propulsion, that it would not be necessary for him to explain further why the Admiralty were desirous, after the Waterwitch trial, to experiment on the jet propeller again in the boat which had been described. The arguments advanced by the gentlemen to whom he had alluded were to the effect that in ships of commerce many accidents to which the screw propeller was liable would be altogether avoided; that many of the perils now incidental to navi-

gation would be removed; and that navigators would have, in the possession of great pumping power, a means of preserving ships and lives which they did not now possess. And they not only considered that that held good with regard to ships of commerce, but that there were special advantages to ships of war which made it still more desirable that the Admiralty should encourage the use of the new method. For his own part, he did not believe in pumping power. He believed that ships would be saved from perils of the sea by proper building, by proper division into compartments, and not by pumps; and even if they passed from the pumps with which they were familiar to the pumps which were supposed to be possible under the new system of propulsion, he thought they would still find that they were going in a wrong direction. But under any circumstances, whatever the advantages might be in the directions that had been pointed out, it was clear that if 2 lbs. of coal were to be burnt instead of 1 lb., for getting a certain result, it was, for the purposes of commerce, entirely out of the question to expect the jet propeller to supplant the screw propeller, or in any way to come into the field for practical adoption.

With reference to the contract into which the Admiralty entered with Messrs. Thornycroft & Co., for the object that had been described, the Board of Admiralty knew that they might expect a loss of about one-half the power, but they were prepared for that, for the sake of the merits which it seemed to them it was possible that such a boat might have. The contract was to give the same indicated H. P. as was given in the boat propelled by the screw, namely, 100; they were also to guarantee that the machinery worked well; they did not guarantee any speed. As a matter of fact, the machinery gave about 160 instead of 100 H. P.; and they had secured a boat which he believed would be found by the trials at Portsmouth to possess so many good qualities that it was very unlikely that they would be called upon to alter the machinery back again to the screw propeller. It was possible that some considerable improvement might be made in the pump, perhaps in the size of the nozzles, and it was possible—he hoped it might be proved to be the case—that there would be a sensibly better result than that which had been already obtained. The boat had the remarkable property of stopping very quickly, but she went astern badly and would not steer—two grave defects. The author had clearly shown the reason for the losses which had to be sustained in that mode of propulsion; not merely had he put before the members in a plain manner the whole of the facts in the case, but he had demonstrated this remarkable circumstance—that there was a screw turbine, a turbine boat it might be called, the turbine being put outside the boat, giving, with the light draught of water which it was supposed was only suitable for the hydraulic propeller, the same speed as could be got with the screw propeller in the ordinary way. The result of the Admiralty experiment, therefore, was a fair boat, capable, possibly, of being considerably improved; an excellent explanation of the causes of the loss in the jet propeller as compared with the screw, and an entirely new propeller, having admirable qualities. It might happen during the time he remained as an adviser of the Board of Admiralty that that department might again be pressed, as they had been during the last twenty years, to build a ship propelled by the hydraulic jet. He could only say, and he said it without the least hesitation, that such a proposal would have his uncompromising opposition.

Mr. Thomas A. Hearson said that in the hydraulic experiments under discussion the short-coming of the propeller was due to losses in the flow of the water through the pump and through the passages, and the loss was much more than could be submitted to with a continued use of the propeller. But the system had its advantages, and its advocates said that if experiments were continued the efficiency would be improved. It was therefore advisable, before continuing the experiments or condemning the system, to examine into the loss quantitatively and see how much was inherent in the system and unavoidable, and how much might be got rid of by improving the apparatus. The author had shown, and it might be accepted without doubt, that 1 ton of water was delivered per second by means of an engine of 167 H. P., with

an efficiency of 0.77, which amounted to this—that every pound of water delivered received an amount of energy of 31½ foot-pounds, in other words, received a head of 31½ feet. Of this head it might be easily calculated that only 10.4 was usefully employed in propulsion, and that 4 feet more was wasted in the energy with which the water left the nozzle. This left 17.1 feet of head wasted in some other way. If the water were imagined to be taken up gradually by the action of the pump, to be endowed with energy given to it by the wheel from the engine, and to be delivered without any abrupt change of velocity into the casing, and then delivered through the nozzles; in other words, if the pumps were assumed to be acting perfectly, how much loss would there be? The loss in such a case—that due to surface friction—was calculable. It might be expressed as a fraction or a multiple of the energy of motion at some point in the line of motion, say at the nozzle. He had calculated that in the Thornycroft boat the fraction of the energy of motion relatively to the nozzle which represented the loss by surface friction was approximately 0.4; in other words, the coefficient of resistance as employed in hydraulics to express the loss of head was approximately 0.4, not less than 0.3 nor more than 0.5. Mr. Thornycroft had succeeded in keeping it down almost to the lowest amount, the boat employed lending itself to a comparatively small coefficient of resistance, for the dimensions were small relatively to the power, and the length of the passages were short relatively to their section. Taking 0.4 of the energy of motion as an accurate estimate, that would amount to 8½ feet of head lost by surface friction, and there remained yet 8½ feet to account for. It would be instructive to work out the loss and efficiency if a value of 0.3 and also 0.5 were assumed instead of 0.4; and further, to work it out for a different velocity of flow. In the Thornycroft boat the velocity of flow was 1¾ that of the boat; what the author called the acceleration was ¾ of the velocity of the boat. If the area of the nozzle was increased 17 per cent., so that the acceleration was reduced to ½, the efficiency fraction was but little altered, being increased from 0.458 to 0.465, calculated from

$$\text{Efficiency} = \frac{2\frac{s}{v}}{(1+F)\left(1+\frac{s}{v}\right)^2 - 1};$$

which was derived from—

$$\text{Efficiency} = \frac{vs}{vs + \frac{s^2}{2} + F \cdot \frac{(v+s)^2}{2}}$$

F being the coefficient of resistance, showing that very little, indeed, could be gained by increasing the size of the nozzle. The best efficiency worked out to 0.52, which was about the limit attainable by this method of propulsion, leaving out of account the efficiency of the mechanism of the engine. If that was good enough as compared with 0.65 of the screw, it might be continued in use; but he thought it would be admitted that it was not good enough. He wished to speak a little more of the loss of 8½ feet, to which he had referred, and which had not yet been accounted for, the additional loss over and above that due to surface friction. From the principle of momentum—the turning moment being equal to the change of moment of momentum imparted to the water—the energy delivered to the water by the engine per lb. of water might be otherwise calculated. It was equal to the product of the velocity of the periphery, and the tangential velocity of delivery divided by G, and equating that to the otherwise estimated amount of 31½ feet of head, it resulted that the water was delivered from the pump wheel with a tangential velocity of but little more than 18 feet per second, from a wheel going at a velocity of 56 feet, into a casing where the water had a much higher speed of motion, since it had, on the average, a velocity of 37¼ feet. It was the eddying due to the difference in the velocities of the streams in the casing that produced the additional loss of 8½ feet—a loss which it was hoped might be got rid of by a better pump. The water came from the wheel into the casing with energy of motion given to it by the engine equivalent only to 5 feet of head. The rest of the 31½ feet was pressure head which took effect in increasing the flow, accelerating the speed of the water in the casing, and the remainder was dissipated in fluid eddying. If it were im-

agined that the streams were kept separate, and the loss for a moment suspended, then the additional 8½ feet would be employed in increasing the flow of the water. Increasing the flow would mean retarding the tangential delivery into a casing where the water would be going faster, which would increase the tendency to eddying. The pump gorged itself. It was easier to find fault than to suggest a remedy; but he would mention that to enlarge the dimensions of the casing might be an improvement, so that the water might flow round the casing with a smaller velocity and not be subject to the eddying described. To fix guiding blades in the passages leading to the wheel might be another improvement, so that the water would come to the wheel with a considerable moment of momentum, and less would be required to be given to it by the pump-wheel itself; or perhaps by making the blades more radial, less curved back, the loss would be less.

Captain H. E. Crozier, R. N., observed that as the officer who commanded H.M.S. Viper in the experimental trials against the Waterwitch, he begged to take exception to some of Admiral Sir George Elliot's remarks. He had never imagined that any comparison could be made between the ships. He had watched the Waterwitch, and it appeared to him that unless she used enormous power, she had very low velocity. When off Portland on her passage to Plymouth in a single-reef topsail breeze, and in the trough of the sea, her propelling force nearly failed. Admiral Sir George Elliot had stated that the vessels Viper, Vixen, and Waterwitch, on account of their rolling, were condemned as unseaworthy; and that the two former had been taken out to Bermuda and laid up there. The Waterwitch was laid up in ordinary at Plymouth, because she was condemned as unseaworthy after failing so signally off Portland, when she just managed to get in "by the aid of her sails" under the breakwater, was taken to Plymouth when the weather moderated, and was paid off. The Viper, on the contrary, was employed on the coast of Ireland and at Liverpool. She steamed about 2,000 miles, exposed to uncertain weather, and then made a passage to Burmuda by sail and steam averaging 6 knots per hour. If she had been condemned as unseaworthy it was scarcely possible that the Admiralty would have allowed her to make such a voyage. At the trials between the Viper and the Waterwitch at Portsmouth, the vessels started with all their weights on board. His orders were that the mean draught of the Viper should be about 12 feet 4 inches. He had an idea that by altering the trim, which he was entitled to do, he should get more speed—a more solid body for the screws to work in; and, at the same time, that by taking the weights from the extremities the ship would be more lively and seaworthy. The builders' trial was 9 knots, but he obtained, as was expected, 9.4 knots per hour. Mr. Ruthven, who was on board, complained that the trial was unfair. The Admiralty decided that another trip should be taken, and he then got 9½ knots nearly. He had stated that at the trials the Viper exerted H.P. up to 650, while the Waterwitch exerted 750. The Waterwitch had then part of her weights taken out, and she ran, not in such a state of efficiency as she should be in when going to sea, at 9.2 knots per hour. He thought that if a vessel so built managed to attain the speed he had mentioned, the same power would be far better exerted by the screw, which was more simple. Again, what would be the use of the Waterwitch in shallow water? If she went into 12 feet 4 inches depth of water, drawing 12 feet 2 inches, she would suck up mud and stones, and remain as if at anchor. As far as her propelling power was concerned, a vessel should go into action under command. Imagine the Waterwitch at full speed, at which alone she would be serviceable, struck by a shot, and some of her pumps getting out of order! It had been mentioned that the Vixen was condemned when she first went outside the breakwater. That was easily accounted for. It was because her hatches were not battened or screwed down. She nearly sank as the water got into her. Of course, when the bottom of a vessel was flat, and she had 2 feet depth of water in her, as she rolled the fires would be put out. When he took the Viper from Plymouth to Bermuda he was thirty days at sea, and there was very little rolling. It was not even necessary to have "fiddles" on the table to prevent the china and glass falling off. In all the 6,000 miles that

he had steamed in the ship he had never seen her roll heavily.

Mr. J. R. Ruthven observed that the author gave the efficiency of the hydraulic in the Waterwitch at 18 per cent., which was a force of only 4,800 lbs. to propel the ship; but to propel the ship at 9.3 knots per hour the hydraulic-propeller must have given out a force of 13,000 lbs., and so have had a total efficiency of nearly 50 per cent. of the indicated H. P. A considerable difference existed in the proportions of the boats whose trials were under comparison. The Viper, Waterwitch, and Swedish boats were all about five times the beam in length; while the Thornycroft was nearly nine times. The author had taken the coefficients from the displacements, but when the proportions differed so much the coefficients from the cross-sections furnished a better comparison of the propellers. They were as follows:

H.M.S. Viper	424
H.M.S. Waterwitch	357
Swedish screw	277
Swedish hydraulic	171
Thornycroft screw	362
Thornycroft hydraulic	160

By these coefficients the Thornycroft screw was shown to be inferior to the screw in the Viper, still more so to the Swedish hydraulic propeller; and the hydraulic propeller in the Waterwitch was 123 per cent. better than the Thornycroft.

Mr. Joseph Bernays stated that he had had considerable experience with centrifugal pumps, but he had not tried to adapt them to the hydraulic propulsion of vessels, because he considered the principle on which they acted was not suitable for that purpose. He did not say that they could not be used; the examples under discussion showed the contrary; but in matters of that kind the question was one of final efficiency or economy. It was understood with regard to the propulsion of vessels that, in order to obtain the best effect, as much water as possible must be driven towards the stern of the vessel, and with as little speed as possible, otherwise a great deal would be lost in energy. The centrifugal pump propelled a small quantity of water with great speed towards the stern. Comparing the two classes of vessels illustrated in Plate 1 the matter would be rendered clear by merely taking the area of the screw and the area of the inlet of the propeller. Judging from the load-line, he imagined that the diameter of the screw was about 3 feet; the area of the circle would be about 7 square feet; the diameter of the inlet of the hydraulic-propeller was probably 1 foot, and the proportion of 0.785 square foot to 7 square feet represented roughly the difference in the volumes of water acted upon, and showed the necessity of greater speed being imparted to that issuing from the hydraulic-propeller than to that which issued from the screw-propeller. Another disadvantage would necessarily be the position of the pump at the bottom of the vessel, where if any damage occurred it would be very difficult to remedy it. It was stated in the paper that floating bodies were not liable to damage the internal propeller, but in reality there was a great sucking in of water into the propeller with a certain amount of speed, which made it liable to catch floating bodies, such as wood shod with iron and things of that kind. If these were to enter the propeller it would be liable to be damaged, and although it was in sight of everybody it could not be got at. If in that way the casing were damaged, he imagined that without bulkheads the vessel would be lost. Something had been said about the great power for stopping leaks, but he did not see where it was to come from, or how it was to be applied, any more than with the ordinary screw propulsion. But assuming that hydraulic-propellers were suitable for the propulsion of vessels, he did not think they had had a fair chance in the arrangements that had been exhibited. Perhaps the preference should be given to the shape of the Ruthven propeller, because even in the paper it was credited with 0.50 efficiency, whereas the more modern arrangement was only credited with 0.47 or 0.46. He did not know whether with regard to the latter any allowance had been made for the improved inlet, because if so the efficiency of the propeller proper would be still less. Many years ago he had written some letters, which were published in some of the engineering papers, with reference to the proper shape of pump-arms. He would not repeat what he had written, but would merely say that the shape of the path which a drop of water should pursue in the pump was

a curve like that stated to be followed in the Ruthven propeller, but not so long; and in order to make the water take that course, the arm must wedge itself under the water near the inlet, and must be more radial as it got out, to let the water issue from the pump-disk in a more tangential direction. He spoke of the matter without any prejudice in favor of one pump more than another. The arms forming the real propelling part of the pump, their shape was of great importance. The position of the pump, as shown in the drawing of the torpedo boat (Plate 1), appeared to him to be altogether wrong. He thought that there were many places where the power of the engine given out was wasted. First, the water was raised vertically, or on an inclined shoot, then it was bent at right angles and round the casing in all directions, whereas it was only wanted to move in one direction. Next it had to go through several small pipes, each bent, and finally it issued through a small jet. At every such change the water lost some of the power that had been put into it. An endeavor should be made to deliver the water out of the pump in as broad a stream as possible, so as to issue with the least possible speed and with the least possible loss of effect; and that, he believed, if other difficulties were not realized, could be done by placing the pump as far astern as possible. How that might affect going astern or steering he would not say, but he thought there might be methods of getting over the difficulty.

Mr. W. Atkinson observed with reference to the intake of water by the inclined shoot, that the author stated that in all previous hydraulic boats the water had been taken through a hole in the bottom, in such a way that all its velocity relatively to the ship was destroyed before it entered the pump, and that that velocity had to be restored by the pump, which involved a large waste of power. It appeared to him that there was a misconception upon that point. In the first place it must be assumed that the water in the river or the sea was stationary, and that the boat only was in motion. As the water was not in motion, it was evident that no energy had been put into it, and that there was no loss to avoid. If it was thought necessary to adopt a scoop in order to bring the water up, then it would be merely a question whether that scoop had better be used in that particular position, having already employed the power of the engine or of the machine to propel the boat, or whether it was better only that the pump itself should draw it up without the scoop. Credit was taken for the fact that in the arrangement described the relative velocity of the water and of the boat was not lost. It was, however, he thought, manifest that if the scoop were taken a little further up, and brought to the vertical line, the water would have acquired the velocity of the vessel before it entered the turbine. It had arrived nearly at that position, and it was evident that the greater portion of the velocity of the vessel would have been given to the water, and if it could not be utilized afterwards by changing its motion, all that energy would be lost. But it was not lost; it was merely the portion due to friction which was lost. If the turbine was lowered almost to the level of the bottom, the scoop done away with, and the hole for taking up the water elongated, then the lifting power of the pump would virtually do all that the scoop did, and this advantage would follow, that the motion of the vessel would not have been communicated to the water; part of the water would not have motion communicated to it, and a certain amount of loss would be avoided. It appeared to him, therefore, that the scoop was a mistake. With reference to the chamber for the distribution of the water into the vanes of the propellers, it would be seen that in the model of a parallel flow turbine exhibited there were guide blades, the object of which was to transfer the pressure of the water into rotary motion, so that when it entered into the chamber of the pump, the inner portion of which was rotating perhaps at about thirty feet per second, it should enter without shock and loss of power. He should like to know why the guide chamber had been omitted, because he had never known any turbine arrangement without one. He thought if it was desired to perfect the pump, an efficient guide-blade chamber should be introduced, and at the top an inverted cone, as was shown in the diagram of the Waterwitch pump, where, however, it was used for a different pur-

pose. There would then be a second guiding power. The water which was forced in a state of great commotion into the chamber would be let off gradually in each direction. It had been stated in the discussion that the object was to drive as much water as possible astern. He thought that the object was not to send the water astern, but to propel the vessel ahead. In driving water astern, the energy due to the motion greater than the motion of the water in the sea was lost. The paper stated that the velocity of the vessel was 22 feet per second, and the velocity of the jet 37 feet. He supposed that the difference between those two velocities was equivalent to 16 H. P. lost. He believed it had been admitted by Mr. Thornycroft that it would have been better not to have had the nozzles at so great an elevation. If the water by that arrangement was raised only 2 feet it was equivalent to a loss of 8 H. P.

That, he thought, would indicate the direction in which there were great and unnecessary losses. If it were really necessary to have that particular form of hydraulic propulsion, he thought it would be much better to place the axis horizontally, which would no doubt get rid of a great deal of choking up the passages, and there would be a direct flow of water through the propeller, and economy would be the result. The axial or parallel propeller appeared to be the best. He wished to direct attention to a turbine with an inward flow, that had, he believed, yielded the highest efficiency of any turbine ever constructed, and no doubt it could be appropriately placed in a vessel of the kind described, and would give much greater efficiency than the one under discussion. Probably one such turbine on each side of the boat would be the best arrangement. It was an American invention, and called the Victor, being a modification of the Hercules. The guide-blade chamber formed the outside casing, inside which was the register rate and the vanes, as in an inward flow, but with the addition of another set of vanes as in a parallel flow turbine. Thus, within a small space there was a large vane surface, with the result that for the same weight a greater power could be developed than in any other turbine. Drawings of it had been given in Emerson's "Hydro-dynamics," 1882, p. 124.

Mr. W. E. Rich observed that the author had quoted the statement of Lord Dufferin's Committee, "We are of opinion that the system is deserving of a more thorough trial than it has yet received," and he thought if the Institution of Civil Engineers were called upon to advise upon such a subject at the present moment, they would have to endorse those remarks made thirteen years ago. He believed that Mr. Thornycroft, who had done a great deal in his first attempt, had not attempted to remodel the vessel or its machinery in any essential particular since his trials were made; and probably if he had to construct another vessel on the same system, many points brought out by the experiments would lead him further on in the right direction. The late Professor Rankine laid down the principle which was now generally recognized, that the chief reason why very high efficiencies could never be looked for from the hydraulic system of propulsion was that another machine was introduced whose efficiency must be allowed for. Mr. Thornycroft had already obtained a speed of 12 6 knots per hour, and from a general review of the calculations given in the paper, Mr. Rich was of opinion that 15 knots was a possible speed with with the same boat and engine power. The great point in which improved results might be obtained was, he thought, in the efficiency of the fan. The author showed an efficiency of fan and engine combined of 0.35, and then estimating that the engines gave 0.77 efficiency in themselves, he credited the pump with the remaining 0.46. But the Thornycroft engines gave a much higher efficiency than 0.77. The traction engines tested by the Royal Agricultural Society at Wolverhampton in 1871 gave an average of 0.84, and the portable engines tried at Cardiff in the following year gave a mean efficiency of 0.825. He did not think that the engines of the Thornycroft boat were inferior to these, for probably none were made with greater care and more frictionless. Taking the Thornycroft engines, therefore, as capable of doing 0.83, the efficiency of the fans would be 0.42. As long ago as 1851 the late Mr. Appold, with the assistance of other gentlemen, determined that one of

his earliest pump-fans, 12 inches in diameter and 6 inches wide, pumping 1,250 gallons a minute, with a 10-feet lift, gave an efficiency of 0.68. A centrifugal pumping engine, for draining Whittlesea Mere, was erected in the following year under Mr. Appold's supervision, and twenty years afterwards, when Mr. Rich assisted in testing it, the engine and pump were giving a combined efficiency of 0.45. A larger engine and pump, which were substituted for the originals shortly after these trials, gave 0.53 of efficiency. The Witham drainage engines and pumps, which he tested in 1873, were much more powerful, and gave a combined efficiency of 0.63, which he believed was about the highest result on record. They were throwing from 350 to 400 gallons per minute, as measured over a gauge weir. Of course, with a centrifugal pump, as with a turbine, there was for every fall or lift one speed, and one volume of discharge at which it would give its maximum efficiency; and if the proper relations between fall speed and discharge were not strictly regulated the efficiency decreased very much. He had not examined the details of the Thornycroft fan analytically, nor was he in a position at the moment to offer any advice as to the best way of improving it; but from the experiments of Mr. Appold and others, he was inclined to think that a broader fan with a smaller diameter, and driven faster, would probably give greater efficiency than the fan exhibited. He also thought that if the skin of the fan could be kept further from the skin of the casing, better results would be obtained. In the construction of fans working centrifugal pumps skin friction was an important element. In the Witham pumps, with fans 7 feet in diameter and 2 feet 2 inches wide, skin friction absorbed a material part of the whole power. There was not only skin friction on the circular surfaces, but there was the skin friction of the water passing through the passages. The result was that a small fan often gave a higher efficiency than a large one. The fan of the Swedish pump resembled that of the late Mr. Appold, but its blades curved more. He had substituted a fan with blades shaped like those in the Thornycroft propeller, about nine years ago, for an Appold fan at a polder pumping station near Amsterdam. The object in putting it in was to get the same quantity of water lifted with a lower engine speed, and this had been attained; but there was no evidence of improved efficiency in consequence of the change.

Mr. W. Schönheyder said it was important, in arranging the propulsion of a boat by hydraulic power, to have a pump of the best shape for efficiency. It appeared, from what had been done with centrifugal pumps by various engineers, that there was a great diversity of opinion as to the best form of pump both in the cross-section and in the shape of the blades. He was certain that if engineers would pay attention to what had been already accomplished with turbines, and would remember that a centrifugal pump was only an inverted turbine, they would not have such poor results as had been from time to time recorded. The best information with regard to the centrifugal pump was, he believed, to be found in Mr. David Thomson's paper, read on the the 14th of February, 1871,* which contained an account of what had been done by the late Mr. Appold. Mr. Appold had experimented upon fans with arms of three shapes, radial straight arms, straight arms placed at an angle with the radius, and curved arms, such as those in the Swedish pump. Naturally he obtained the best results with arms like those in the Swedish pump, but that was no proof that that was the best possible section. The object, of course, in constructing arms should be to give the water a gradual rotary motion, cutting through it like a knife through a piece of cheese on entering, and then gradually getting the rotary motion, and finally giving it the proper direction in which it was wanted to run. That, of course, was the reverse of what was found in turbines. In the blades experimented with by Mr. Appold, the curve was that of a screw of uniform pitch. The water being moved around by those blades at once acquired a certain rotary motion, and no increase of rotary motion was given by them afterwards. The effect was a blow to the water, which must very much diminish the efficiency, in accordance with what had been laid down

* Minutes of Proceedings Inst. C. E., vol. xxxii., p. 26.

by the best authorities on turbines. The Thornycroft wheel was a better pump than the Swedish pump or the Waterwitch. Mr. Thornycroft had shown the path which the water took in passing through the wheel, but unfortunately he partly condemned his own work. It would be seen that the water entering the vanes took first of all a radial direction, or rather a radial direction partly backwards, and when it had passed halfway through the vanes a rotary motion was given to it gradually. From the diagram it appeared as if the vanes were made, he scarcely knew how, and afterwards the path which the water took was found; instead of which, first of all, the path of water should be determined, and then the vanes made of a proper section to give it that path. It appeared as if the first half of the length of the blades was useless, or worse than useless, because the water passed through them, and lost a certain amount of efficiency in friction. If half the inner length of the blades was cut away, he believed that better results would be obtained, motion would be imparted to the water in the same way, and there would be less frictional resistance. He did not know how far the diagram of the Waterwitch pump was correct—no doubt it was correct from the data possessed by Mr. Thornycroft, but probably the delivery had been very much underrated. It would be seen that the water received a sudden blow immediately it entered the arms, which it ought not to have, and which would necessarily occasion loss. Mr. Thornycroft was correct in stating that Mr. Rigg was the first to use guide-blades for a propeller, but not in stating that he used curved guide-blades, for they were perfectly flat. The water was assumed to strike against them like a billiard ball against the cushion, and fly off at the same angle. A paper by Mr. Rigg on the subject was read before the Society of Engineers in 1868,* and the honor of first using that method was certainly due to him. As to the possibility of a water-propelled boat ever being efficient, he thought there could be no two opinions. Instead of putting a propeller into the water to act upon the whole sea, it appeared to him to be necessary to take the sea into the boat in order to get a sufficient volume to act with efficiency.

Mr. J. P. Symes said that reference had been made in the paper to a floating fire-engine in which, by the advice of Mr. Brunel, one of the hydraulic propellers was used. Some of the previous speakers had expressed an opinion that if the axle were placed horizontally it would give better results than if it were vertical; and one speaker had said it was a pity that the hydraulic propeller had not been tried on a boat of exactly the same dimensions. Messrs. Thornycroft in their experiments had varied the dimensions of the boat somewhat, for the purpose of giving an advantage to the hydraulic propeller. The floating engine referred to in the paper was still in existence. It was fitted with a centrifugal pump with the axle horizontal, and was driven by means of gear, but did not exist at the present as a hydraulic-propelled boat. This vessel had been built by Messrs. Ditchburn and Mair in 1855, and the machinery, including the centrifugal pump for propelling the vessel, the steam fire-engine and the gearing for driving the centrifugal pump, were fitted by Messrs. Shand, Mason and Co., the engines being high pressure, but the results obtained were not very satisfactory as to speed. The boat was 100 feet long, 14 feet 9 inches beam, and had a mean draught of 4 feet 4 inches. The design of the boat was very favorable for a moderate speed—a much higher speed than had ever been obtained by the hydraulic propeller. Water was taken in at the flat bottom through a square hole without any particular guide. Originally the water was discharged below the water-line, which some persons seemed to think an advantage; but some time after the jets were removed and placed a short distance above, as described in the paper, but the difference in speed was scarcely perceptible. He was not able to say whether the jet protruded outside the vessel; if it did, that no doubt would account for there being no difference in the speed, because the friction of the jet outside would be sufficient to re-act against the advantage of placing it below the water. The speed obtained by the boat, as far as he had been able to ascertain, was never higher than 6 knots per hour. There was always great difficulty in keeping the gear

* Society of Engineers. Transactions for 1868, p. 202.

in order, so much so that at last the vessel was towed to fires instead of using the hydraulic propeller. In the year 1878 the Metropolitan Board of Works, which had charge of the fire brigade, decided to have the vessel converted into a twin-screw boat. Here was a vessel of the same dimensions, with the same displacement and draught of water forward and aft, and by converting it into a twin-screw boat an opportunity would be afforded of seeing if any advantage would be gained. His opinion was that Mr. Thornycroft in altering his design had done so in favor of the hydraulic propeller. He could not give any indicated H.P., but the basis might be taken as boiler power. It was proved that the engines driving the centrifugal pump used all the steam that could be generated by the boilers; and as the boilers were left in, they had the same basis of power. Two pairs of high-pressure engines were fitted in by Messrs. A. Wilson and Co., of Vauxhall, with cylinders 8 inches in diameter and 9 inches stroke, the propellers were 3 feet in diameter and 5 feet 6 inches pitch, from two hundred and sixty to two hundred and eighty revolutions per minute were attained. On a trial with the twin-screws a speed of 10 knots per hour was obtained against 6 knots for the hydraulic-propeller with the same boat, the same displacement, and the conditions in fact almost the same. He thought this confirmed the author's opinion and experiments that even with exactly the same boat the results would be much in favor of the screw-propellers for the power developed by the engines, so much so that the hydraulic-propeller had no chance in the competition.

Mr. Sydney W. Barnaby, in reply, said he thought the institution as a body had reason to congratulate itself upon not being responsible for some of the facts and opinions that had been advanced during the discussion. So many criticisms had been made upon the paper, that he should not reply to them all. Of course, with a large hole in the bottom of the boat, it would be likely that floating material would get in, but to prevent that a grating had been supplied, and although it checked the admission of the water, it had the advantage of keeping anything out except a certain amount of sand and gravel, which the pump threw out in very shallow water. He could not approve of the suggestion that the pump should be at the stern of the boat. The reason for putting it in the center was plain; the object was to get as big a pump as possible, and as the boat was small, the center was the only place for it. There was a further benefit from the extra displacement obtained by the increased section of the boat abaft the inlet. The further forward the pump was taken the greater the advantage of that extra displacement. It had also been said that the pump would be more efficient if it had guide-blades. The fans of the pump were so adapted as to take the water without shock, and to gradually accelerate it, and he thought when the pump was so devised the guide-blades were not necessary. With respect to his coefficient of efficiency of the engines, he had taken some pains to get at a fair estimate. He had written to Mr. Rich, knowing that he had made many experiments, and he gave him a probable efficiency of about 0.80. Mr. Lilliehöök, the designer of the Swedish boat, had taken his efficiency as low as 0.72, and Mr. Brin, the Italian constructor, about the same. Mr. Wright gave 0.77 as a more probable efficiency for a marine steam-engine. It should be observed that although the Thornycroft engine was no doubt as efficient as any other, still in the trials the engines were quite new in the boat (after a little running no doubt they would go more freely) while those of Mr. Rich were running in their most efficient condition. It was, in fact, a trial for economy. With regard to the use of the scoop for preserving the velocity of the feed, the water had a velocity relative to the boat, equal to the speed of the boat. If the water was admitted to the pump by a hole at right angles to the direction of its motion, all that velocity was instantly destroyed. In other words, the water had a forward velocity imparted to it by the boat equal to the speed of the boat, out of which velocity no advantage could be obtained. When the entrance was a curved one, as in the Thornycroft boat, the water had its direction gradually changed, and it might be made to rise into a tank at a height depending upon its velocity. True it

would have a forward velocity equal to the speed of the boat if the pipe reached the vertical; but it would be the original velocity with a changed direction. It might be carried any distance in the tank, but when it was allowed to fall through a pipe curving aft it would issue with the same velocity, neglecting friction, with which it entered.

In reference to what had been said about the arms of the pump appearing to be ill-adapted to the path of the water as calculated, it should be remembered that a very slight variation in the quantity of water altered that path considerably. The pump was discharging rather more water than was intended, and it illustrated the fact that a turbine could only work at maximum efficiency in one particular set of conditions. The speakers in favor of the system of hydraulic propulsion might be roughly divided into two classes: there were first those who said they admitted that there was a considerable loss of power by the system, but looking at the great advantages which were to be obtained from it (advantages which had been described in the paper and which he thought might be easily over-estimated), the question was what would they pay for them—would they sacrifice something in speed, or would they keep the speed and make sacrifices in armor, or guns, or cargo, as the case might be? That was a question for the naval architect and the shipowner to determine, and he had nothing to say upon it. But the other class said not only that there need be no loss from the adoption of hydraulic propulsion, but that there should be a gain. Most of those speakers had commenced by saying that they did not approach the subject from a scientific point of view, and they proceeded to lay down a set of novel and astonishing principles as a basis from which to attack the results that had been arrived at. He could only say that he had endeavored to approach the subject from a scientific standpoint. He did not know whether Admiral Selwyn was one of those who made that modest disclaimer, but one of the most important, but certainly the most misleading of those novel principles was that enunciated by him when he said that the velocity of discharge was a much more important thing than the quantity; that the quantity did not matter in the least in regard to the velocity obtained. Taking this principle as a basis, he endeavored to show that by fixing the velocity of discharge at 37 feet per second, Mr. Thornycroft had thereby limited the speed of the boat to that obtained. He stated he was sure he could not obtain a speed of 17.7 knots per hour without a velocity of discharge of 91 feet per second. Mr. Barnaby submitted some figures in order to invite attention to that point, and to show the fallacy of the principle thus laid down.

$$\text{Efficiency of jet} = \frac{\frac{wvs}{g}}{\frac{wvs}{g} + \frac{ws^2}{2g} + \frac{fwv^2}{2g}};$$

w = weight of water discharged in lbs. per second;

v = speed of vessel in feet per second;

s = slip or acceleration;

g = 32.2 feet per second.

	Thornycroft Boat.	Hypothesis I.	Hypothesis II.
w.....	2210	221	22100
v.....	21.4	21.4	21.4
s.....	15.85	158.5	1.585
$v+s$.....	37.25	179.9	22.985
Efficiency.	0.71	0.23	0.77

At the top was the formula given by Prof. Rankine for the efficiency of the jet. He would therefore only repeat that the first term in the denominator was useful work, the other two terms were lost work. It would be seen that in the second term w appeared in the first power only, while s the acceleration was in the second power. Therefore, if the velocity of discharge was increased the loss was increased as the square. If the quantity discharged was increased the loss increased at the same rate only. The first column showed the quantity of water acted on and the rate of discharge in the Thornycroft boat. The efficiency was 0.71. In the second column one-tenth the quantity was assumed, and ten times the acceleration, the useful work, and therefore the speed of vessel, remaining the same. The efficiency was 0.23. In this case the indicated H.P. would require to have been increased as 23 to 71. In the third column there was ten times the quantity and one-tenth the acceleration,

giving an efficiency of 0.77 with the same propelling effect. In these hypotheses no account had been taken of friction, which added seriously to the loss as the velocity of discharge was increased. Therefore the rate of discharge was not so important as the quantity discharged, and if the whole Thames had been pumped through the boat it would only have required a velocity of discharge infinitesimally greater than the speed desired. He had heard that Admiral Selwyn had built a hydraulic boat of his own. It would have been interesting to the Institution if he had explained how he had applied his principles, and with what success. He regretted that the discussion had been confined to the old battle-field of the Waterwitch and the Viper. He did not care to enter into that battle-field. If the results had proved conclusively that jet propulsion was wrong, there would have been no need for a Thornycroft hydraulic-boat or any other. They did not prove anything conclusively, and it was not until he came to collect all the information available that he found out why that was so. No experiments of any value had been made as to the quantity of water pumped, or as to the rate of discharge; it was therefore impossible to get accurately at what the duty of the pump was. He did not pretend that the efficiency of 0.18 was absolutely correct. As he had previously explained, he had taken that from the calculations made by Mr. Brin in a paper read before the Institution of Naval Architects (vol. xii.). From considerations of the probable resistance of a vessel of the size and shape of the Waterwitch, he calculated that 0.18 of the H.P. which was indicated would be sufficient to give the speed which she attained. Mr. Barnaby had been induced to take those figures almost upon faith. Mr. Ruthven, junior, had, he thought, proved too much; he had endeavored to show that the screw of the Viper was a great deal better than the screw of the torpedo-boats, but that was so obviously impossible that the rest of his calculations upon the same basis were probably incorrect. The new boats did really give a fair means of comparison. There had been no prejudice one way or the other, either in regard to the Thornycroft or the Swedish boats. In each case the designers of the screws designed the turbines, and the results had been unchallenged. The coefficients which he had given for the Viper and the Waterwitch were supplied to him by the Admiralty, and they represented trials where the displacement and the speeds were as nearly as possible alike. In the tables which Mr. M. W. Ruthven had exhibited, he had mixed up the coefficients of trials where the displacement varied from 995 to 1,205 tons, and where the speed varied as much as 1½ knot per hour; he had mixed them all up and divided them by the number. He would refer to Fig. 13 to show what the effect of that was. In that diagram was the curve of the coefficient of a torpedo-boat at the same displacement under different speeds. The influence of a difference of 1½ knot could be seen. At 9 knots there was a coefficient of 228, and at 1½ knot more speed it had fallen to 160. What was the value of any comparison between two systems of propulsion if carried out upon those two speeds, and if besides the displacements and forms were different? The attempt to make such a comparison betrayed a fundamental misconception of the meaning and value of coefficients of performance. Similar vessels of the same length should be tried at the same speeds and at the same displacements if a comparison of propellers was desired, and similar vessels of different lengths should be tried at corresponding displacements and corresponding speeds. By corresponding displacements he meant displacements varying as the cube of the length; by corresponding speeds, speeds varying as the square root of the length. Mr. Ruthven found a discrepancy between Table 1 and Table 2. He did not understand how the coefficient of the Waterwitch could be so much better than the coefficient of the Thornycroft boat, and nevertheless the efficiency of the hydraulic machine in the latter boat could be so much better than that of the Waterwitch. But there was not the smallest connection between the coefficient of performances in Table 1 and the efficiency of the hydraulic machine in Table 2. If the engines of the Waterwitch were put upon a Thames barge, or if the Waterwitch herself were moored head and stern the efficiency of the hy-

draulic-machine might be exactly the same, but Mr. Ruthven would not like the efficiency of his system of propulsion to be judged by the coefficients of performance obtained in such a trial. Admiral Sir George Elliot had claimed that jet propulsion had never had a fair trial, and that the designers had always been hampered by the conditions imposed by the Admiralty. If he had led him to believe that they had been hampered in any way by conditions as to the draught of water he had misled him. The only conditions imposed were those stated by the Director of Naval Construction, namely that the boats should be as nearly as possible the same, that a similar boiler was to be used, and that the designers were to do their best as far as getting speed was concerned. Every suggestion which they had made as to modifications in the form of the trial boat which they considered would be to its advantage had been cheerfully assented to by the Admiralty. Some of the suggestions were undoubtedly prejudicial to the efficiency of the boat as a torpedo-boat, but as it was a trial of hydraulic propulsion the designers were permitted to make even those modifications. One of them, a small one, was in the position of the boiler; it was carried out to the end of the boat, and far away from the steam impulse for ejecting the torpedoes, which was an objectionable feature, but which was considered advisable for other ressons. Another was the position of the discharges. If the discharges had been below water the noise would have been avoided, and also the serious objection arising from the large amount of white water which made the boat readily detected by the electric light at night. They had, however, fitted up a small centrifugal pump in a launch with the object of determining the best position for the discharges. The nozzles were attached to dynamometers, so that the reaction could be measured. The experiment showed an enormous resistance caused by the nozzle when below the surface; and it was clear that unless long discharge pipes were used carrying the water aft, as in the Swedish boat, where the nozzles were below the surface (Fig. 2), the discharge must be above water. Admiral Sir George Elliot had also said that the screw-boat should have been reduced to the same draught of water as the hydraulic. As a matter of fact the draught of the screw-boat under the keel was less than that of the hydraulic-boat, and it was only the blades of the screw revolving below the keel which made it exceed the draught of the hydraulic boat. Admiral Sir George Elliot would cut off the legs of the screw-boat, and make it run upon its stumps, in order that it should not outrun the hydraulic. That was not fair, but even on that ground the screw-turbine propeller would beat the centrifugal pump. As had been previously stated, in second-class torpedo-boat, with 4 inches less draught of water than the hydraulic now had, nearly 5 knots additional speed per hour could have been obtained. The little boat, of which a model was exhibited, only drew 12 inches of water, and it reached the astonishing speed of 15 miles per hour with a screw-turbine propeller. A great deal of the uncertainty connected with the subject had been produced by loose statements as to the performances which had been attained by hydraulic-propelled vessels. It had been said that the Nautilus had beaten another vessel, the Volunteer; it had been stated perhaps what the speeds of the vessels were, and their H.P., but nothing was mentioned about the displacements, or which had the advantage in point of form. It was asserted, too, that the Nautilus challenged all the Citizen boats on the river, and that they declined the contest. That, however, proved nothing. He might have come before the Institution and said, "Here is a hydraulic-propelled boat two-thirds the length of a Citizen steamer, with very much less displacement, with a boiler of only two-thirds the heating surface and a weight of machinery of only 5½ tons against 14 tons, and yet it can run round and round any Citizen boat on the river." That would have been the truth and nothing but the truth, but not the whole truth. The whole truth was that by means of forced draught an enormous power in proportion to her size was realized. It was a great pity that statements should be put forward which proved nothing, but which led to a great deal of misconception. He thought it was most desirable that scientific questions of that sort should be treated in a more scientific manner.

Sir J. W. Bazalgette, C. B., President,

said that if the Institution, as a body, did not hold itself responsible for the views which had been expressed, he felt sure that the subject had been advanced considerably by the discussion which had taken place. He had no doubt that it would lead to further experiments, and that an increase in the rate of propulsion by different modes would be the result of the consideration that had been given to the subject. The members must also feel that the author had devoted great attention to the subject, and had made a very able reply to the many objections which had been raised in the course of the debate.

ON THE SOLAR TEMPERATURE QUESTION.

By F. GILMAN.

Written for Van Nostrand's Engineering Magazine.

I would like to make one more statement (which is my last) in regard to this question, since Prof. Wood's article in the February number of this Magazine contains some assertions which call for a reply. He says I assume that the intensity, or temperature, will be directly as the quantity of heat passing a given area of the spherical shell, but as this is erroneous my demonstration fails.

Even supposing, however, that the ether is perfectly diathermanous, and consequently that the temperature of space is zero, it will not affect the question in the least; since what we wish to determine is the temperature of a material body like that of which the sun is composed, when brought into the sun's vicinity.

If such a body when brought near the sun will have a temperature of more than a million degrees, no reasonable person would deny that the temperature of the sun must exceed this.

The temperature to which a body will rise when exposed to a given quantity of radiant heat, depends on its specific heat and on its absorptive power.

A body brought near the sun would be converted to the gaseous state, and since the specific heat of all gases is nearly the same, its rise of temperature would be proportional to the quantity of heat transmitted to it, if its absorptive power did not change.

Admitting, however, that the absorptive power of solar substance is very small, and that if conceived to be detached from the sun's surface, it would absorb but a very small fraction of the heat directly radiated to it, this would not invalidate the argument for the high solar temperature. The experiments of Prof. Tyndall have proved that the absorptive powers of different gases follow precisely the same order as their powers of radiation. Hence, the smaller the absorptive power of solar substance, the smaller will be its radiative power, and the higher must be the temperature of the sun in order to radiate the same quantity of heat. If, on the contrary, we suppose the absorptive power of detached solar substance to be very great, so that it absorbs nearly all the heat radiated to it, then it is easily shown (if the law of inverse squares from the center be true) that its temperature due to direct radiation from the sun, when near its surface, will not be less than two million degrees Fah.

Prof. Wood attempts to show that according to the law of inverse squares as applied to a point on the surface of a sphere, the temperature of this point will be infinite, and since, when the entire spherical surface is involved and the inverse squares are reckoned from the center the temperature is finite, therefore the law of inverse squares from the center reduces to the absurdity of giving a smaller temperature when the entire surface is involved than when only a point is considered.

It is evident that any process of reasoning that leads to the conclusion of an infinite temperature must be fallacious, even though that conclusion be derived from a formula.

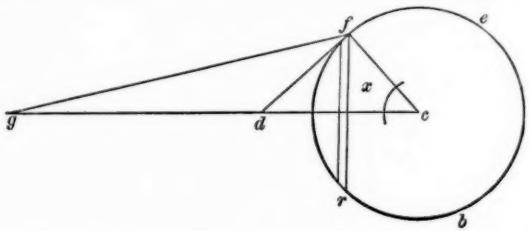
There are many formulas which, while correct in general, utterly fail to give true results at certain limits. If, however, we consider an actual point as radi-

ating heat the formula will not give an infinite temperature at the point; for though the distance from the point be infinitely small, the amount of heat radiated is also infinitely small, and the expression for the temperature reduces to $\frac{0}{0}$, an expression which may, and in the present case does, have a finite value.

Prof. Wood says: "The nearer a body is to the sun the less will be the hot surface to which it will be exposed. So that while we may admit that the heat received from the small portions of the sun exposed to the pyrometer varies inversely as the squares of the distance from them, it also varies directly as the sum of those small portions; and since this sum diminishes as we approach the surface of the sun, the law of the inverse square from the center fails."

In reply to this I submit the solution of the following problem:

Assuming that the intensity of heat radiated in any given direction from a hot surface varies directly as the sine of the angle of obliquity, and inversely as the square of the distance, when the radiating surface is a point, it is required to find the relation between the intensity of the heat at a point g, and the distance gc of that point from the center of the hot spherical body feb.



Consider an infinitely small zone fr, and representing fc by R, and the angle fcd by x, the area of that zone will be 2π R sin. x Rdx=2π R^2 sin. x dx. Draw fd at right angles to fc. Then we shall have for the measure of the intensity of heat received at g from the infinitely small zone, fr, the following expression:

$$dH=\frac{2\pi R^2 \text{ sin. } x\ dx \text{ sin. } gfd}{gf^2}$$

But

$$gf^2=gc^2+R^2-2\,gc\,R\cos.x$$
$$gc^2=gf^2+R^2-2gf\,R\cos.gfc$$

And since cos. gfc = − sin. gfd, we have

$$\text{sin. } gfd=\frac{gc^2-gf^2-R^2}{2\,gf\,R}$$

Substituting these values we obtain for the measure of the intensity due to the infinitely small zone

$$dH=\frac{\pi R \sin.x\ dx\ (2gc\,R\cos.x-2R^2)}{(gc^2+R^2-2gc\,R\cos.x)^{\frac{3}{2}}}$$

$$H=2\pi R^2\int\frac{(gc\cos.x-R)\sin.x\,dx}{(gc^2+R^2-2gc\,R\cos.x)^{\frac{3}{2}}}$$

This integral taken between the limits $x=o$ and $x=\cos.^{-1}\frac{R}{gc}$ will include every portion of the spherical surface that can radiate heat to the point.

Let $a=gc^2+R^2$ and $b=2\,gcR$

Then

$$H=\frac{2\pi R^2 gc\int\cos.x\sin.x\,dx}{(a-b\cos.x)^{\frac{3}{2}}}$$

$$-\frac{2\pi R^2\int\sin.x\,dx}{(a-b\cos.x)^{\frac{3}{2}}}$$

$$\int\frac{\cos.x\sin.x\,dx}{(a-b\cos.x)^{\frac{3}{2}}}=-\frac{2}{b^2}\left\{(a-b\cos.x)^{\frac{1}{2}}+\frac{a}{(a-b\cos.x)^{\frac{1}{2}}}\right\}$$

$$\int\frac{\sin.x\,dx}{(a-b\cos.x)^{\frac{3}{2}}}=-\frac{2}{b\,(a-b\cos.x)^{\frac{1}{2}}}$$

Restoring the values of a and b, and taking the integral between the limits cos. x=1 and cos. $x=\frac{R}{gc}$ and reducing, we have

$$H=\left\{\frac{2\pi}{gc}(gc-\sqrt{gc^2-R^2})\right\}$$

But

$$-\sqrt{gc^2-R^2}=-gc+\frac{R^2}{2gc}+\frac{R^4}{8gc^3}+\frac{R^6}{16gc^5}+\ldots\ldots$$

Hence

$$H=\frac{2\pi}{gc}\left\{\frac{R^2}{2gc}+\frac{R^4}{8gc^3}+\frac{R^6}{16gc^5}+\ldots\ldots\right\}$$

When gc equals or exceeds 2R, all terms following the first may be neglected, and we shall have $H=\frac{\pi R^2}{gc^2}$; that is the intensity varies inversely as the square of the distance from the center; while for all values of gc between R and 2R the application of the preceding formula will give even a higher value for the temperature than the simple law of the inverse squares. Therefore it is evident that if the law of the inverse squares holds for a point, it may be said to more than hold for the center of a spherical body, as far as the argument for the high temperature of the sun is concerned.

MEASUREMENT AND FLOW OF WATER IN DITCHES.

By ROSS E. BROWNE, Member of the Technical Society of the Pacific Coast.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

In the April number of this magazine there appears an article with the above heading, by Chas. E. Emery, Ph.D., in which some unaccountable misstatements occur. It is asserted that Mr. Bowie has used in his calculation the *average depth* of the stream in place of the *hydraulic mean depth*. That Mr. Bowie has not been guilty of such stupidity is apparent from the simplest calculation. The diagrams, giving dimensions of the ditches named, and included in the original publication of the Technical Society, were omitted in the magazine article. This fact makes Mr. Emery's method of reaching conclusions, with regard to the coefficients of flow of these ditches, a mystery.

Mr. Emery says: "Full dimensions are not given from which to calculate r on the correct basis, but, assuming probable proportions for the ditches, it will be found that for the Texas Creek Branch Ditch, c should be 109, instead of 59* as stated, &c."

It is presumed that Mr. Emery has carefully read Mr. Bowie's paper, as published in the January number of this magazine, and was aware of the fact that the values of c were determined from actual measurement of the quantities flowing through the ditches. It is distinctly stated (page 35), in connection with the Milton ditch, that c was "determined from the gauging at the measuring box." It is also presumed that there was no intention of ignoring these measurements.

* 33, not 59.

If, then, the Texas Creek Branch Ditch is taken as example, a statement is found to the effect that the sectional area is 13.5 (square) feet, the grade 20 feet per mile, and the flow 32.8 cubic feet per second. If under these conditions the value of c is to be 109, as claimed by Mr. Emery, the "probable proportions" must be assumed such as to make

$$r=\frac{Q^2}{a^2c^2s}=\frac{(32.8)^2}{(13.5)^2(109)^2(\frac{20}{5280})}=0.131 \text{ ft.}$$

hence the wetted perimeter

$$=\frac{a}{r}=\frac{13.5}{0.131}=103 \text{ feet.}$$

The cross-section corresponding to such a wetted perimeter and an area of 13.5 square feet, would have approximately either a width of $102\frac{7}{10}$ feet and a depth of $\frac{13}{100}$ foot, or a width of $\frac{26}{100}$ foot and a depth of 52 feet. Such "proportions" do not seem "probable."

The small values of the coefficient, obtained by Mr. Bowie, are plainly due to the significance given to c. This c is evidently intended to guide one in *projecting* such ditches as are mentioned, and "covers all common sources of loss" such as sharp bends, leakage, diminution and variability of cross-section. It was plainly Mr. Bowie's purpose to furnish the means of estimating, upon the basis of the projected dimensions of cross-section, the capacity of a ditch of the character described, after the natural changes have taken place and it has become more or less permanent under reasonable attendance.

EXPERIMENTS UPON THE HARDENING OF PORTLAND CEMENT.

By M. PERRODIL.

Translated from "Annales des Ponts et Chaussées."

The experiments herein cited were made upon specimens of Portland cement from the works of Lonquety and Demarle, at Boulogne-sur-Mer.

The percentage composition of this cement after the complete expulsion of water and carbonic acid was

Silica	23.8
Particles of sand	1.3
Alumina	8.9
Ferric oxide	2.0
Lime	63.6
Magnesia	0.4

Eighty-eight cubes of this cement were prepared without sand on the 12th of August, 1883. Half of the number were immediately immersed in water. The other half were left exposed to the air.

At different periods indicated in the following table, four cubes of each set were subjected to a crushing strain. The fragments were immediately analyzed to determine the amounts of water and of carbonic acid that had entered in combination.

which acquire in the water a considerable degree of hardness; 2d by the action of calcium hydrate upon basic calcium silicate.

The proportions of silica alumina and lime found in the cement permit the formation of these compounds, and assuming that both the silica and alumina play the part of acids and unite with different proportions of the lime, there may result one of the groups represented by the following formulas:

$3\,Si\,O_2$. $4\,Ca\,O$	53.46
$Al_2\,O_3$, $Ca\,O$	13.74
$Ca\,O$	29.10
	96.30

or

$Si\,O_2$ $2\,Ca\,O$	67.45
Al_2O_3 $3\,Ca\,O$	23.40
$Ca\,O$	5.45
	96.30

After twenty-four hours the immersed cement contains about the same amount

Age of the specimen.	Sample in Water.		Sample in Air.		Breaking weight, per sq. centimeter.	
	pr. ct. of water.	pr. ct. of $C\,O_2$.	pr. ct. of water.	pr. ct. of $C\,O_2$.	Set in water.	Set in air.
					k.	k.
1 day	6.4	0	6.1	0	43.6	33.6
3 days	8.7	0	6.2	1.8	119.2	84.0
7 days	9.2	0	6.0	1.8	141.6	104.9
15 days	12.8	0	8.0	1.0	163.4	122.8
1 month	15.4	0	6.5	2.8	234.2	103.0
2 months	14.6	0	7.6	4.0	293.4	123.8
3 months	11.2	0	4.3	6.0	231.2	126.6
6 months	15.3	0	8.3	5.8	329.6	171.6

M. Fremy explains the hardening of cement:—1st by the formation of an aluminate of lime represented by one of the formulas:

$Al_2\,O_3$, $Ca\,O$; $Al_2\,O_3$, $2\,Ca\,O$; $Al_2\,O_3$, $3\,Ca\,O$

and the subsequent formation of hydrates of combined water as the cement in the air. Neither of them contains carbonic acid. The amount of water in combination corresponds to about three-tenths of an equivalent, taking the lime present as one equivalent.

At the end of a month the amount of water in the immersed cement has con-

siderably increased, amounting to three fourths of an equivalent, while in the cement left in the air it has remained nearly the same.

The carbonic acid, always absent in the immersed cement, increases gradually in the cement exposed to the air, until at the end of three months the amount is sufficient to indicate the presence of 7.64 parts of lime. Now the amount of lime in the second of the above sets of formulas is somewhat less than this, so that the composition of cement hardened in the air is represented by proportions somewhere between those represented above, but nearer the second set than the first.

A remarkable result exhibited by the table is the relation between the increase of combined water, and the increase of strength of the cement. These figures are all the more striking from the fact that a diminution of the water percentage is attended with a diminution in the breaking weight.

The conclusion that may be drawn from these experiments is:—1st, that the cement acquires a greater degree of strength in the water than in the air; and 2d that the water which enters into combination is an important factor in the hardening process, whether it forms a hydrated calcium aluminate, or establishes a more complex reaction with the free lime and then upon the silica and alumina so as to form an artificial pozzuolana.

A FLASHING TEST FOR GUNPOWDER.

By Prof. CHAS. E. MUNROE, U.S.N.A.

From the Journal of the American Chemical Society.

Among the methods in use for the determination of the condition and quality of gunpowder, is the "flashing test" According to the *Ordnance Instructions U. S. Navy*, p. 345, "about eight drams of powder are poured on a glass plate so as to form a conical heap, and 'flashed' by applying a hot iron; no residuum should be left and only a few smoke marks should be seen on the plate." Capt. Smith, R.A., in his *Handbook of the Manufacture and Proof of Gunpowder*, p. 83, proceeds in the same way, but he places the powder in a thimble-shaped, copper cylinder, "which is then inverted on the flashing plate. This provides for the particles being arranged in pretty nearly the same way each time, which is an all-important point in flashing. The decomposition of the powder will be more thorough if it be thrown together in a conical heap, than if it be spread out in a thin layer on the plate, hence, for comparison of different powders, they should be placed on the plates as nearly as possible under the same conditions.

"If the powder has been thoroughly and effectually incorporated, the small charge placed on the plate will 'flash' or puff off when touched with a hot iron, leaving only smoke marks on the plate. A badly incorporated paper will, on the other hand, leave specks of undecomposed saltpeter and sulphur, and will therefore give a dirty residue. But the 'flashing' test, though apparently most simple, is one which, like the examination by eye and hand, requires experience to enable the observer to form an accurate judgment. Though a very badly incorporated powder could be detected at once, it is by no means easy to judge between two powders, both tolerably good, as to which has undergone the most thorough incorporation. Flashing should therefore be constantly practiced with all classes of powders, and it is useful to keep some samples of bad powders to flash occasionally for comparison. Powder which has once been subjected to and injured by damp will be found to flash very badly, no matter how carefully its incorporation may have been performed. This arises from a partial solution of the saltpeter having taken place, causing a consequent disturbance of the incorporation."

Comdr. J. D. Marvin, U. S. N., in his *Objects and Resources of the Naval Experimental Battery*, p. 18, repeats the above directions and suggests weighing the plate on which the flash has been

made, but, as he provides no means to prevent the absorption of moisture and oxygen and the escape of the hydrogen and ammonium sulphides, the method is of no value.

In the *Comptes Rendus* 78, 1138; 1874, Col. Chabrier proposes, what he terms a *pyrographic* method for the examination of gunpowder and a detailed account is given in the *Revue d'Artillerie*, 4, 396; 1874, of its application by the *Comité de l'Artillerie* in determining the relative value of wheel mills, stamp mills and *moulins à tonneaux* in effecting incorporation and of the length of time necessary in each case to produce the desired result.

This method consists in flashing the powder on sheets of paper, colored blue with iodide starch. Sheets of uniform tint, 0.30 meter long by 0.15 meter wide, are dampened and placed on a plate of glass of the same size. A half gram of powder is then trailed on the paper, following the longer axis. It is flashed by a red-hot iron wire and it is found that the center of the blue paper is bleached, while black spots and streaks appear on the white ground and white spots on the blue ground. The size and shape of the bleached space and the number and arrangement of the spots and streaks are determined by the character and amount of the powder used. Col. Chabrier does not give the rationale of his process but it is to be inferred from the fact that he styles these results *pyrographic* images, that he believes the bleaching to be due to the heat evolved by the combustion. The well-known experiment of the bleaching of starch paste, colored blue with iodine, by heating in a test tube, is an example of the same kind.

This process is an advance upon the older one, but in applying it some years ago I found it difficult to prepare papers of the same degree of blueness and that the evanescent character of the color made it difficult to preserve the test papers intact for any considerable length of time; so, as before, we must either practice the method continually or else flash powders, which we have kept as standards for comparison, with each set of tests we make, in order to arrive at any good results. Or, finally, we may photograph the test papers, but this involves considerable labor and the loss of the color.

Since the flashing test is the simplest, readiest, and in the hands of an expert, the best test for the incorporation of powder, and, since it also fairly indicates the amount of deterioration which a powder has undergone during transportation and storage, it has seemed to me desirable to seek some method by which the record could be made permanent. Such a record could then be filed at the factory with the other data concerning a given powder, or, in the case of the Government, they could be inclosed with the quarterly returns of the inspecting officers, at different stations, to be examined by some expert in the bureau. Specimens of the tests of standard powders could also be furnished inspecting officers, to guide them in the interpretation of the results of their tests, and, finally, a sample of the required test might be attached to the specifications for a gunpowder to be purchased.

After some search I believe that I have secured such a permanent record, by employing a paper colored with Turnbull's Blue, upon which to make my flash. This paper is the same as that used in the "Blue Print Process" of photography, and is easily procured in commerce, The use of the paper was suggested by the following facts. When gunpowder burns, the reaction which takes place may, according to Debus, *Proc. U. S. Naval Institute*. 9, 76, 1883, be represented by the reaction $16\,KNO_3 + 13C + 5S = 3K_2CO_3 + 5K_2SO_4 + 9\,CO_2 + CO \times 8\,N_2$.

Since, however, in ordinary gun powders there is more carbon and sulphur than is required by the above equation, secondary, endothermic reactions take place, which may be combined and represented as follows: $3K_2SO_4 + 2K_2CO_3 + 7C + 7S = 5K_2S_2 + 9C\,O_2$.

Further, on exposure to the air, the polysulphides formed are oxidized to thiosulphates. I have observed that, in my experiments, the characteristic smell of the latter was most noticeable when the powder was badly incorporated.

It is well known that solutions of the alkalies and the alkaline carbonates decompose Turnbull's Blue and thereby destroy its color. Advantage has been taken of this reaction to increase the distinctness of "Blue Prints," or to make

such additions to them or alterations in them as desired. With this I exhibit a specimen of the blue paper, upon which one of the above reactions is written by the aid of a solution of potassium carbonate. The alkaline sulphides and thiosulphates also act upon the blue paper, but with less intensity and with the partial production of a yellow color. By flashing gunpowder then, upon such paper, yellow and white spots will be formed through the action of the substances formed by the reaction.

The test is made as follows: Pieces of the paper, from 15 to 20 centimeters square, are dampened and placed on a sheet of glass or copper. A truncated leaden cone 3 centimeters in capacity is closed with the finger at the smaller end, filled evenly with powder and inverted on the paper. The result is a conical heap. The heap is immediately fired, either by a hot iron or copper wire, or, as in my experiments, by a fine platinum wire, heated to incandescence by an electric current. The paper is exposed to the action of the residue for thirty seconds and then immediately placed under the spigot and washed with running water. When pulverized gunpowder cake is used it will be found that the space described by the base of the cone, has been blackened and partially bleached by the dampened layers of powder in contact with it; that about this space are black smutches and streaks, and that the whole surface of the paper is marked by white and yellow dots. Where the powder is badly incorporated the spots are coarse, and irregular in shape and distribution; where the incorporation is complete, the spots are fine and quite uniformly distributed over the surface so that the paper appears but of a paler blue, with occasional spots and few streaks.

With this, I forward specimens of tests made with powdered "mill cake." All of the specimens belong to the same "charge," but the first was drawn after the mill had been running four hours; the second, at the end of eight hours; the third, after twelve hours; and the fourth, after sixteen hours. The latter is known as the "finished composition." This length of running is rather unusual, but the charge used at the mills is greater than common. The tests exhibited were made October 19, 1883. I have yet others made April 26, 1883, which are to-day apparently as fresh and distinct as when made. It is believed that the papers show what is described above. That importance is given, in interpreting the results, to uniformity of the bleaching and in the arrangement of the spots, depends upon the fact that gunpowder is a mechanical mixture, and, therefore, that the regularity of the combustion and the uniformity of the accompanying reactions must depend upon the fineness of the ingredients and the intimacy and uniformity of the mixture. If the ingredients are coarse, and the mixture imperfect, the combustion will go on slowly and irregularly, and the resulting globules of residue will be of considerable size and be deposited near the center of action. If the incorporation is complete, the reaction will take place nearly simultaneously throughout the whole mass, and the globules will be, as a rule, quite small and projected to some distance. This interpretation is for mealed powders having the same formula. I have not yet been able, personally, to extend my experiments to granulated powders or powders of varying proportions and ingredients, but I believe that this test will form a useful method for the study of these powders.

In order that the indications may be interpreted aright, it is necessary that the conditions under which the experiments are made, shall be as nearly uniform as possible, and the first of these is, that the color of the test paper should be in all cases as nearly as possible of the same depth. The paper may be purchased in an emergency, but it varies among manufacturers owing to the many different formulas according to which it is made. Among these I have selected that issued by the Penn. R. R. Co. for use among its operatives.

"Take 10 oz. (283.5 c. c.) of clean water and put it in an opaque bottle, add 1¼ oz. (35. 44 grms), of Red Prussiate of Potash, allow this to dissolve. In a second containing 6 oz. (170. 1 c. c.) of water, put 2½ oz. (70. 88 grms), of Ammoniocitrate of Iron, allowing this also to dissolve. Add the second liquid to the first and shake thoroughly. Keep closely stoppered and not exposed to light."

"In a room, from which daylight is excluded, but where lamp or gas light may

be used, the paper to be printed on is laid on a table, and the fluid applied with a *clean* sponge. Care should be taken to apply the fluid as evenly as possible, and every part of one side should be gone over. For that reason it would be well to sponge the paper, first in one direction and afterwards crosswise to the first. When a sheet is sensitized it is put away in a drawer to dry, but never place one sheet on the top of another before they are dry; afterwards it makes no difference. Sensitized paper may be kept in a drawer for a week or more, without injuring its sensitive quality.

"In using the fluid, care should be taken to pour out no more than is needed for the time, as it would be apt to spoil the fluid in the bottle if any fluid which had been used, was poured back again.

"For the same reason the saucer into which the fluid is poured, and the sponge with which it is applied, should be washed out immediately after using and also before using."

For the purposes of this test for gun-powder the dry sheets are now exposed to strong sunlight for four or five hours. When about to use, immerse in running water for five minutes, lay on the plate of glass and remove the excess of moisture by aid of filter paper or a blotter. The paper must be thoroughly moistened but without "standing" moisture.

Since writing the above I have received from Lieut.-Commander W. M. Folger, U. S. N., commanding the Naval Experimental Battery, the following statement concerning the testing of a granulated powder by this method.

"In firing a sample of experimental powder lately, I had reason to believe, from its performance in the gun, (caliber 6″), that the powder was badly incorporated. Tested in the manner you suggested with Turnbull's paper and following all your directions, indications were furnished which (when compared with results obtained with a normal sample of approved powder) verified most definitely the value of the method you suggest."

SWIFT CRUISERS.

From the "Nautical Magazine."

It is to be earnestly hoped that the Admiralty will well consider what they are about before allowing themselves to be committed to the very questionable policy of building for the protection of our commerce, swift cruisers, of upwards of 400 feet in length, and of such other extreme proportions as have been proposed to them by a leading firm of shipbuilders.

A large Government order for vessels of this type would be a great help to the present needy condition of the shipbuilding trade; but the first question to be considered is—are such ships really wanted? It appears to be pretty generally admitted, that if war were to break out, and this country were involved, the Admiralty could not, effectively, protect the large trading British fleets from capture; therefore, it may be argued, ships of some kind are assuredly required. Allowing all this, naval men, and seamen generally, are far from being assured that such large vessels as have been proposed would be the most suitable for this important service. If it has not actually been experienced, it may very readily be assumed, that in naval warfare all vessels of great length have many objectionable points. Speed, without doubt, in this connection, is a most necessary qualification, but it is not everything; and before an order is given for such very large and costly ships, it should be clearly understood whether a high rate of speed cannot be got out of much smaller and less costly vessels.

This country can afford to spend twenty millions to protect and maintain her ocean commerce, and when that twenty is finished she should resolve to lay down another hundred millions at the back of it in order to carry out that policy; but though spending money freely and lavishly, she cannot afford to spend foolishly, or upon ships that may turn out to be failures before they have ceased to be new.

It has been said of two long and large ships, which were, to some extent, the outcome of a panic—the Warrior and the Black Prince —and which were laid down in somewhat of a hurry; that amongst other serious defects, they would neither wear nor stay. Perhaps the English of this was that they took double the time they ought to, in performing the circle—a most important evolution in naval war.

Certainly, long and heavy ships work under great disadvantages as battle ships of any kind, and particularly as cruisers, where rapid movements and ready changes of position are so all important. The greyhound is a fast dog, but he frequently loses the hare by his inability to double as quickly as his quarry; and he would turn out to be an entirely useless animal when pitted against a rat.

Apart from the question of her enormous cost, a cruising gunboat of 450 feet in length would quite as easily succumb to the first lucky shot from an enemy, as would a vessel of half her tonnage, being also cellular and equal in everything excepting speed. She would, indeed, be more easily hit in a vital part, from her inability to turn as rapidly as the smaller ship. The proposition, it appears, is, that nothing under 450 feet, and a corresponding breadth and depth, can be made effectually fast, and invulnerable to the heavy shot which are now thrown by ships of all nations; but this argument requires confirmation by more than one authority.

There are several cases on record in the history of shipbuilding where great speed has been obtained out of very moderate proportions, independent altogether of the twenty-five miles an hour of torpedo boats, and the following is one of these cases taken from a well-known periodical:—"September, 1882. A new steamer, named the Mary Beatrice, built by Messrs. Samuda Brothers, Poplar, for the South-Eastern Railway Company, and engined by Messrs. John Penn & Sons, of Greenwich, has had a journey down the river for the purpose of making her official trial trip. The dimensions of the vessel are as follows:—Length, 255 feet; breadth, 29 feet; depth, 15 feet 6 inches; draught, 8 feet 1 inch; and tonnage, 1,063 burthen. The indicated horse-power of the engines is 2,800. She is distinctly an advance on her sister vessels, Albert Victor and Louise Dagmar, built by the same firm for the South-Eastern Railway Company, two years ago, embodying several valuable improvements in her arrangements for comfort of passengers, but notably in her additional length of five feet, whereby room and displacement have been obtained for the introduction of larger boiler and surface condensers, to which additions the excellent results obtained are mainly attributable. The usual four runs were made on the Maplin Sands, giving the extraordinary mean speed of 19.019 knots, or something over 22 miles an hour." Thus we have here a distinct practical refutation of the doctrine that high speed and great length, or great tonnage, must necessarily go together.

We have had it illustrated often enough in the trials of torpedo boats, although there is this difference in regard to them, that they are too small to be made invulnerable to heavy shot.

In this little dilemma, it will be well to remember that neither naval constructors nor eminent shipbuilders are infallible in this direction, as they are not, also, in other matters connected with naval architecture.

Some years ago, when Atlantic competition began to set in rather strongly, it was thought by all builders and constructors in this country—and perhaps by all others in any other country, for nobody said nay!—that great length and narrow beam were everything in the direction of speed. This idea continued to prevail amongst all who had anything to with ships, and who gave expression to their opinions; but about five or six years ago owing to the still greater demand for larger ships, and the restricted depth of water upon certain dock sills and bars of harbors, it was found necessary to increase the beam instead of the depth, and with results which surprised nobody more than the designers themselves. The assertion now made, and apparently just discovered by naval constructors and others, that to obtain great speed, great tonnage is indispensable, may be correct, or it may not, but if it be erroneous it only reminds us of other positive assertions made when iron steamers were in their infancy, and to the effect that, respecting length, anything over 300 feet was encroaching upon the confines of

danger. From 250, however, an advance was quickly made to 300—made by men of action, who had a more powerful belief in their own abilities, than in the opinions—eminent or otherwise—of the men of thought. Results have proved that eminent *savants*, scientists, and professors, have shown themselves to be somewhat blind leaders of the blind. Thus in direct opposition to the learning and doctrine of such professors, practical men have impatiently advanced not only to 300 but to 600, and, without any single instance, as far as is known, of the terrible consequences predicted, being fulfilled. This digression only shows what an amount of groping in the dark there has been by the leaders of thought in this science, and how careful one should be before accepting statements coming even from eminent, or supposed to be eminent, personages, who are not also practical men.

One may easily be within the limits of truth in asserting that, during the last 30 years, few iron steamers have been built which have not been found conspicuously faulty, or capable of great improvement before they had ceased to be new. This applies even with greater force to new ships, not as fighting machines only, but as specimens of naval architecture and seaworthiness, as floating bodies. In this important question of cruisers, the Admiralty should invite three or four of the first shipbuilders of the country to take a contract for their construction—say one each, to four builders—and the equally important matter of length, tonnage, and mode of propulsion, to be settled by a committee, composed of builders, architects, and naval commanders. Perhaps as little of science as possible, with its mystifying technicalities, and as much practical knowledge as possible, might, with profitableness, be imported into their deliberations.

What they would have to consider mainly would be,—can great speed, combined with fighting power, be got out of vessels of this class, having *moderate* instead of excessive dimensions, and if not, why not? To non-scientific people, or to those who are neither naval architects nor shipbuilders, it appears somewhat singular that if twenty-five miles an hour can be got out of torpedo boats, twenty-two miles an hour out of passenger ships of the tonnage of the Mary Beatrice, and twenty-one miles out of 400-footers, like those which traverse the Atlantic between Liverpool and New York, why twenty, at least, cannot be obtained for fighting ships ranging from 1,500 to 1,800 tons.

There are difficulties in the way, no doubt, in combining moderate tonnage and high speed with invulnerability to shot, but in this age of inventions those difficulties should be far from insurmountable.

In the case of the Mary Beatrice we have the tonnage, and we have the speed; thus, if other conditions are satisfactory, it only remains to make her shot proof, or moderately so; to be entirely so would be next to impossible. It would be as unreasonable to expect ships to come out of an action unscathed, as to expect soldiers to leave the battle-field without a wound, and perhaps the sooner we do away with such a forlorn hope, the better it may be for us.

As long as naval wars continue, we shall have ships foundering as of yore, independent of all their armor, and it is just possible that the time is not far away when this contrivance will be given up for ships, as it was for men, years ago.

As long as guns are made which can penetrate armor, it is doubtful wisdom, indeed, to build very costly armored ships; and the nation which possesses a great fleet of small gunboats, carrying heavy guns, with great speed, is a nation to be not lightly esteemed, nor gratuitously insulted. The handiest and perhaps the best ships for this service, should have a length from 260 to 280 feet, a suitable beam, and a draught of water not exceeding 18 feet, with all stores and provisions on board. They should be either twin-screws or double-enders, with a propeller forward as well as aft, unless it should be found that strong engineering difficulties are in the way of such a plan.

Costing something like £70,000 each, we should, at such a rate, have three, against one, of the large type proposed—or, for a fleet, sixty *versus* twenty. Even if their speed did not come up to that of the others by a knot or two, a dozen of such ships would be immeasur-

ably superior, and more effective, than half-a-dozen of the others. Let them sail in couples, with two guns each, of great penetrative power. It must be remembered that their primary business will be neither chasing nor running away, but standing by the fleet under their protection. This fleet will be slow sailing, not reaching above 9 to 9½ knots per hour, and what they will require of their great speed, will be to endeavor to choose the fighting position when the enemy puts in an appearance, and to endeavor to sink him without losing sight of their charge. Thus the necessity for a long run would not be sustained. If their object was to chase everything that hove in sight, we should, sooner or later, find the enemy sending in decoys ahead of the actual fighting ships, to prey upon the defenceless fleet, after the gunboats had gone sailing away in chase.

Had the Americans sailed their ships in fleets, with good strong guards, which they could easily have supplied, the Alabama's career of burning and sinking would have speedily come to an end, for she must either, in such a case, have faced the enemy, or kept out of the way.

If, after the breaking out of war, it were found necessary to employ such large vessels as have been proposed, what could be easier, than for the Admiralty to buy on charter a few Atlantic steamers of great speed, and keep them, under such terms, till they find them unnecessary for that service. But the proposal to saddle the country with a fleet of very costly ships, which will be unserviceable after a war is finished, and which also may never be wanted, is not wise. Such vessels as have been suggested in this paper would not only be effective, but also cheap ships, and would be cheaply maintained, as their crews would be small. They would be admirably adapted for general service during peace, and being light-draughted would find all the harbors of the world open to them.

There are probably few people taking any interest in naval matters who will not remember the exploits of the turret ship Huascar on the West coast of South America, during which she withstood the attack, and returned, with interest, the fire of two powerful British frigates. The Shah was one of the frigates, a ship which cost perhaps half as much again as the Huascar. It is believed on that coast that she actually beat our two ships; but what is certain is that they not only failed to capture her, but did not even cripple her, as the Admiral intended. It is said that the Admiral would have sunk her, and tried to do so, but after witnessing the method of her fighting, was very glad to give the Huascar a wide berth—he was afraid of being rammed by her. This ship was manned by a scratch crew, and officered, so it is said, by a landsman—who happened to have a pretty strong head, and some smattering of artillery practice.

Now, what would this vessel have done had her crew and officers been British? To have fought her more gallantly would have been impossible; but to have sunk both the Shah and the Amethyst would have been probable.

The Huascar was a brig-rigged vessel of about 200 feet in length, and a tonnage ranging somewhere about twelve hundred. She was protected with about four and a-half inches of armor, and her greatest speed was under eleven knots an hour.

REPORTS OF ENGINEERING SOCIETIES.

American Society of Civil Engineers—March 18th, 1885.—Vice-President G. S. Greene, Jr., in the chair. A paper by Mr. D. J. Whittemore, past President Am. Soc. C. E., on Roofing-Slate was read. The writer used, in 1872, a large amount of Pennsylvania black slate for roofing some freight houses in Chicago. The slate seemed to be of fair quality; but in about six years thereafter, disintegration occurred to such an extent that it became necessary to remove nearly the whole of it.

In 1879 there was required a large amount of slate for shop buildings for the Chicago, Milwaukee and St. Paul Railway, and proposals were received from a number of firms for its supply. Seventeen varieties from as many different quarries were received, and in order to judge of their quality, experiments were made as to the comparative strength when submitted to transverse stress, as to the relative avidity for water as shown by capillary attraction, as to the relative specific gravity, and as to the relative absorption of water when immersed. Two slates of each variety were given to one assistant, and two to another, for the purpose of testing, and they were directed to give for each slate a distinctive number in a scale of seventeen parts. Both assistants agreed in their determinations as to the position of each slate with reference to this scale. The assistants were not acquainted with the price of any of the slate. From the determinations thus made a table was formed by the writer, giving in separate columns the figures determined, and in another column the figures showing the cost

of each variety, and an addition of these figures was considered as fairly showing the relative positions of the different varieties.

The slate offered at the lowest price gave the best aggregate, and was purchased. It was a green slate from Vermont, and has been used upon a large number of buildings, some of which have been in service over five years, and no complaint as to quality has been made. The writer is not prepared to say that the deductions thus made can be in all respects considered a fair relative exponent of quality, but in this instance the decision is apparently justified by experience.

A paper by Mr. William P. Shinn, M. Am. Soc. C. E., was then read in reply to a paper by Edmund Yardley, M. Am. Soc. C. E., in discussion of Mr. Shinn's previous paper on Railway Efficiency. Mr. Yardley suggested that the average movement of cars on foreign roads has, contrary to Mr. Shinn's supposition, increased since 1868, and not diminished. To this Mr. Shinn presents the statement—also prepared by Mr. Yardley—showing that the mileage of cars absent from the Pittsburg, Fort Wayne and Chicago Railroad has increased as stated; yet that, as this company is leased to the Pennsylvania Railroad, it is scarcely fair to include the Pennsylvania as a foreign road, and deducting the cars counted as absent which were upon the Pennsylvania Railroad, the movement on actual foreign roads is diminished and not increased.

Mr. Yardley does not consider that there is any necessity for the new system of blanks proposed in Mr. Shinn's previous paper; to which it is replied that, by the use of that system the change can be accomplished, and the per diem plan put into operation, without any addition to the expense at present incurred in keeping trace of cars, provided that the charge for cars is made sufficiently high to operate as a penalty for their detention.

Mr. Yardley criticised the rate proposed: in answer to which Mr. Shinn referred to his paper published in the Transactions for June, 1883, page 215, where he suggests at the rate five cents per ton per day, reckoned on the capacity as marked on the cars. This method is important on account of the many and widely different tonnage capacities—cars running from five to thirty or thirty-five tons. The suggestion of furnishing cars upon the basis of the annual cost of repairs plus the interest on the cost, divided by the number of working days in the year in order to reach a per diem rate, is lacking in equity.

A letter was quoted, illustrating a marked case of increase in the efficiency of railroads to transport freight occurring in Holyoke, Mass.

Mr. Shinn suggested that if the car accountants of the country would apply the experience which they had to the consideration of the facts in their possession and within their reach there would result an early action. The fact that, during the past year of dullness in all traffic, many railroad companies increased their car equipment in the face of the wholly inadequate movement of cars, needs explanation, and the policy which led to the increase needs correction.

ENGINEERS' CLUB OF PHILADELPHIA—RECORD OF REGULAR MEETING, March 21st, 1885.—President J. J. de Kinder in the chair.

Mr. E. S. Hutchinson proved geometrically that the well-known Latitude and Departure Method of Calculating Areas was true for all positions of the assumed axis or meridian, provided due regard be had to the signs; from which it was obvious that tedious computations could always be very much shortened by conceiving the meridian to pass through a convenient point within the area, instead of through the most easterly or most westerly point, as given in the books.

Mr. John Wood, Jr., visitor, exhibited handsome working models of cars and tracks equipped with the Curtis & Wood Automatic Car Coupler, and fully explained its workings. Various tests of the model were proposed and successfully made. The inventors claim that this coupler insures absolute safety in coupling and uncoupling cars, a great saving of links and pins, and much time in making up trains; that it can be applied to cars with little or no greater cost than the ordinary draw bar, which, in fact, is used, provided with an automatic steel hook instead of the ordinary pin, which hook has about double the strength of the pin now in general use; that it has all the advantages of the present link and pin coupler as to coupling to the ordinary draw-head or any other having a link or provided with a pin; that it can be applied to any freight car without changing the ordinary draught rigging, bolt or springs; that accuracy of coupling with the links in any possible position is assured by the construction of the bull-head throat, which guides the link when forced backwards to the proper position. When it is desired not to couple, the uncoupling lever (at outside end of end sills) is thrown up, thus placing the crank in an opposite position and making it impossible to couple.

The Secretary presented, by title, for Prof. L. M. Haupt, a revision of his paper upon Scales of Maps, which Prof Haupt has prepared for the Club Reference Book.

The discussion on strengthening the West Main Abutment of Chestnut Street Bridge was continued by the author of the paper, Mr. J. Milton Titlow, Prof. Haupt, Mr. Herring and Mr. Howard Murphy.

RECORD OF REGULAR MEETING, April 4th, 1885.—President J. J. de Kinder in the chair.

The Secretary presented, for Mr. Jacob H. Yocum, an illustrated description of the recently constructed Water Works at Columbus, Ga., which city has a population of 25,000. The Chattahoochee River was investigated as a source of supply, but on account of the expense of filtering after its frequent freshets, and of pumpage, it was abandoned, and a gravity system adopted. Among the adjacent hills were found a pure and soft water, delivered through the gravel beds, and a gathering ground of twelve square miles which would yield, after allowing 50 per cent. for absorption and evaporation, a daily supply of 15,000,000 gallons. The water is impounded in two successive dams, respectively 130½ and 115½ feet above the center of the city. The upper dam is 266 feet

long by 21 feet high; area 20 acres capacity, 100,000,000 gallons. The lower dam is 250 feet long by 21 feet high; capacity 20,000,000 gallons. The forest ground they occupy was carefully cleared, grubbed, and surface removed to the gravel and clay. The discharge of upper into lower dam is arranged with reference to aeration of the water

The water is conveyed to the city by 18,009 feet of 12 inch main, which divides, at the river, into two 9 inch wrought iron pipes laid under the floor girders of a bridge 800 feet long. These pipes unite in a 12 inch main again upon the city side. It is intended to substitute a submerged main for this double pipe.

The distribution consists of 10, 8, 6 and 4 inch cast iron pipes, fitted with the Cassin double fire-hydrant and the necessary valves.

A 1 inch jet can be thrown 85 feet. At the opening test 7 streams were thrown 75 feet simultaneously. The works provided abundance of pure, good water during a four months' drought, and have generally exceeded expectations. An additional 400,000,000 gallon reservoir, is, however, contemplated, to meet pros pective requirements.

The discussion of Strengthening the West Main Abutment of the Chestnut Street Bridge was continued.

PROCEEDINGS OF THE ENGINEERS' CLUB OF ST. LOUIS.—ST. LOUIS, APRIL 1, 1885.—The Club was called to order at 8 P. M. by Vice-President McMath, thirty members and four visitors being present.

Minutes of last meeting were read and approved.

Executive Committee reported that the resignation of Mr. E. Harrison and Mr. Samuel Rockwell had been accepted; also, that C. D. Lamb and J. C. Meredith had forfeited their right to membership by non-payment of initiation fee.

The amendment of Sec. 7, of the bye-laws, proposed at the last meeting was adopted by a unanimous vote.

The following gentlemen were proposed for membership: Mr. O. A. Orrman, St. Louis, Mo., by Messrs. J. A. Ockerson and C. W. Clark; Mr. Walter S. Russel, Detroit, Mich., by Messrs. W. B. Potter and J. A. Ockerson.

Prof. F. E. Nipher read a paper on "The Efficiency of a Pair of Holtz Machines, one acting as generator and the other as motor," which was discussed.

Mr. K. Tully read a paper on "Construction in Wood and Iron."

A general discussion followed.

ENGINEERING NOTES.

THE RIVER HUMBER.—The estuary of the Humber extends from the Trent Falls, at the confluence of the Trent and the Ouse, to Spurn Point, a distance of thirty-eight miles. Receiving the drainage from an area equal to one-fifth of the whole of England, and with tidal conditions probably unsurpassed in any other large river or estuary, it is pre-eminently adapted to carry a great commerce. Unlike he Mersey, with its 8 feet or 9 feet over the t bar at low water, it has a deep low-water entrance channel of 60 feet to 70 feet available at all states of the tide. This needs no dredging to keep it open, and so powerful is the scouring action that in spite of a million tons of mud being thrown into the river every year, no ill effects have been observed. Hull is the third port of the kingdom, the annual value of its imports and exports being 40 millions, while those of Liverpool and London are, respectively, 206 and 200 millions. Grimsby's trade is 12 millions, and that of Goole 9 millions.

At Trent Falls the Humber is about one mile wide; at Hull it is about two miles, and at Spurn, some four miles at high water, but there are places of greater width than this. In its lower portions the Humber forms a favorable harbor of refuge for coasting vessels, and it is said that as many as 700 to 800 sail have been anchored in the roadsteads at one time. From Spurn Point up to Hull there is a deep low-water channel, such that the largest ship in the British Navy may navigate it, and anchor off the town at any state of the tide. Of course any hostile ship could do the same, and the Stallingboro' fort and Paul battery opposite would prove but a poor defence in the absence of naval reinforcements. But although vessels can reach Hull at all times, they can only enter the docks when the tide serves. The tide at Hull depends upon many causes besides the attractions of the sun and moon, indeed, in this river the direct action of these bodies is scarcely felt. In the open ocean the crest of the tidal wave travels approximately at the same rate as the earth rolls. But as soon as the narrow seas of the British Isles are entered, the wave loses its distinctive character and travels in the different channels at very reduced and varying rates, and with many peculiarities. This tidal *wave* is essentially different from the tidal *current*. The speed of the wave is that at which the crest or high-water mark travels, but the speed of the tidal current is the actual velocity of the water. The average speed of the tidal wave between Spurn and Hull is twenty-two miles an hour, that is, high water at Hull occurs about an hour after high water at Spurn, and the distance between them is twenty-two miles; but the actual motion of the water never exceeds five miles an hour. The great tidal wave arriving from the Atlantic is broken up on reaching the British Isles, and divides north and south, the velocity being reduced from many hundred miles per hour to thirty, forty, or fifty miles. The part which passes round Scotland helps to form the tidal wave in the North Sea, passing the mouth of the Humber in a S.S.E. direction. There is thus no direct impulse into the estuary as there is into the Severn, and consequently the range of tide is not excessive. At the same tme the Humber meets the sea at a point where the tidal wave has its maximum oscillation, and consequently tides of 20 ft. to 22 ft. are of usual occurrence at Hull. Assuming the rate of flow in the river to be four miles an hour, it is evident that the tidal current cannot travel more than twenty-four miles, and yet the river is tidal up to Naburn Lock in the Ouse, a distance of 80 miles, where high water occurs four hours later than at Spurn, by which time nearly all the water

which entered the estuary from the sea has gone out. This is due to the impulse caused by the ingress of the tidal water in the first twenty-four miles, and this impulse is transmitted forward in the same way that an undulation may be sent along a rope by shaking one end of it.

The Humber is an exceedingly turbid river, and it is a debated point whether this silt comes down with the land water, or is carried in by the sea. The advocates of the latter theory point to the fact that thirty miles of clay cliffs between Bridlington and Spurn are wasting at the rate of 2¼ yards per year, and that the material is borne southward by the tide to the mouth of the river ready to be carried into it, and they strengthen their cause by showing that during a freshet, when the land water is abundant, the river is clearer. Those who favor the opinion that the mud comes from the up-country, show that the cliffs are carried away on the ebb tide, when the detritus goes out to sea, and not to the mouth of the river. The clearness during a freshet they ascribe to the fact that some of the affluents of the Ouse come from a part of the country that does not provide mud, and consequently the increase of total water in the stream may be greater than the increase of suspended alluvial matter. The author is one of those who recognize the turbidity of the Ouse as being due to material carried down by the stream. On account of the continual to-and-fro flow of the tidal water, an object moving with the current only makes five miles net progress towards the sea in a fortnight. It therefore follows that in each five miles length of the river there is concentrated fourteen days' detritus from a water-shed of 10,500 square miles, and this should amply account for its muddiness. Analyses also show that at the turn of high-tide the amount of suspended matter is least, and at low tide is geatest.

It is intended to carry out some improvement works in the Ouse by training the channel, but these, the author believes, would have no effect upon the amount of tidal water in the river, and would not lessen the scour below Hull. The proposed works in the Humber, he fears, would not be so harmless. The present deep channels in front of Hull are dependent, he holds, for their very existence upon the maintenance of the existing natural conditions in the Upper Humber, and particularly of the south channel past Chalders Ness. Now the conservancy lines would shut up the present channels and substitute a new one through the sand banks, to serve both for ebb and flood. This trained channel would terminate a good way above Hull, and below it the water would follow its own course, probably with the result of altering the existing condition of affairs opposite Hull. Fortunately there is no immediate prospect of the works being commenced.

Goole and Grimsby are both increasing their trade, while Hull remains nearly stationary. But there is hope that better times will come soon. There are great works in progress by which the tidal facilities of the Humber will be fully utilized. Ships drawing under 18 feet of water will be able to enter the new Alexandra Dock at low water, so that a majority of the vessels frequenting the port will be able to pass in without any delay.—Abstract of a paper read before the Hull Literary and Philosophical Society, by A. C. Hurtzig.

IRON AND STEEL NOTES.

PRODUCTION OF BARS, WIRE, &c., DIRECT FROM FLUID STEEL.—A patent is published in Germany (*Zeitschrift des Vereins deutscher Ingenieure*, January 10th, with an illustration) taken out by C. M. Pielsticker and C. G. Muller, for a process and apparatus the object of which is to produce bars, wire, &c., direct from fluid steel by pressing it out through dies in a manner somewhat similar to the production of rod and pipe from lead. An iron vessel is lined with refractory material and provided with a manhole at one side and a cover at the top, both capable of being securely closed. At the bottom at one side, opposite the manhole, there is attached by bolts a cast-iron outlet pipe. Through this passes a steel tube with a water space around it exactly like a water tuyere, so that the inner tube can be kept constantly cooled by circulation of water in the space. The end of the steel tube, on the inside of the container, is constructed to hold a fireclay end, or nozzle, where the liquid steel comes in contact with it. The container being already hot, a steel bar is passed through the steel tube so as to project through the fireclay nozzle into the container. Liquid steel is then run in, the cover secured, and the container connected by means of a tube at the top with a vessel containing liquid carbon dioxide. The steel bar is then withdrawn through the tube or die, and the pressure of the carbon dioxide forces the fluid steel to follow it. so that it is drawn out as a continuous rod, of the dimensions and shape of the die, to be passed at once still red hot, through rolls to finish it as required.

A NEW PROCESS OF STEEL MAKING.—A patent has recently been granted to Mr. W. A. Otto Wuth, of Pittsburgh, Pennsylvania, for a new process of producing steel from wrought iron with plumbago, the steel produced being, it is stated, of a high grade and practically free from sulphur and phosphorus, while containing a definite percentage of carbon. The process consists in making the steel from wrought iron that is practically free from phosphorus, sulphur and carbon, by melting it on an open hearth in contact with a form of carbon which will not oxidize at the heat necessary to smelt the iron, but which will unite with the iron at that heat. Any wrought iron which is sufficiently low in phosphorus may be used as the basis of this process, though muck bar made according to a previous patent of Mr. Wuth is preferred as being especially pure. The decarburized iron of the requisite purity is first cut into pieces of convenient length and placed in the hearth of the furnace in layers piled one above the other. Between each layer of iron is spread a thin stratum of plumbago, preferably in a pulverized condition, although it may be used in lumps. For this purpose the inventor employs the plumbago of commerce, but of good quality, containing

about 98 per cent. of carbon and 2 per cent. of silica, with a trace of iron. The hearth of the furnace is charged with alternate layers of wrought iron and plumbago, until it is sufficiently full, the relative thickness of the layers depending upon the amount of carbon which it is desired that the resulting steel should contain. If the iron were absolutely free from slag, and the plumbago were also absolutely pure carbon, then the proportions of the charge would be the same as the proportions of iron and carbon in the desired steel product; but as there is always more slag in muck bar (not, however, exceeding 1 per cent.), and some silica in the plumbago, the proportions of which can be ascertained before the furnace is charged, the necessary allowance for these elements will have to be made, and the percentage of carbon can be regulated with great exactness, so as to produce steel, it is claimed, very nearly approaching to an ideal steel. The furnace being thus charged with muck bar and plumbago, the charge is melted in the usual way, and the operation further carried on as in the well-known open-hearth process. Before the melted metal is withdrawn from the furnace a small and definite amount of speigeleisen or ferromanganese is added. By this process Mr. Wuth says he has made several charges of steel of 12 tons each containing as low as 100th of one per cent. of phosphorus, and that he will be able to produce any kind of fine steel that may be desired.

RAILWAY NOTES.

SOME rapid tunnel driving has lately been done on the Mersey Tunnel Railway, by Colonel Beaumont's boring machine. The distance accomplished last week, through the red sandstone under the Mersey, was 87 yards, which is the "fastest on record." The heading now being driven, and which is nearly completed, has a total length of about 950 yards; and this, as well as the previous heading of about 700 yards in length, are intended for effecting the ventilation of the main tunnel. The total distance driven by Colonel Beaumont's machine—which cuts a circular heading rather over 7 feet in diameter—in connection with the Mersey Tunnel, is about 2,250 yards, which includes the first operation, viz., the boring of the drainage heading.

THE Midland Railway from Woodlesford, near Leeds, to Barrow-in-Furness was obstructed throughout Sunday. A correspondent says:—Messrs. Cammell and Co., of Sheffield, recently completed an immense steel propeller for a steamship, which is being constructed at Belfast. The blades of the propeller—one of the largest yet made—were so wide that they overlapped the opposite line of rails to that on which the propeller was being transported. On Sunday arrangements were made for the conveyance of the propeller from Woodlesford to Skipton, and in order to effect this the passenger trains along the route were shunted and blocked to allow the special train to pass. At the stations and junctions the propeller excited great interest. The train journeyed to Skipton safely, where it was intended that it should remain until next Sunday, but it afterwards proceeded to Carnforth, and subsequently to Barrow, where it arrived on Sunday night.

ORDNANCE AND NAVAL.

THE NEXT BIG GUN.—According to the *Morning Post*, preparations are being made at Woolwich Arsenal for the proof trials of an enormous gun which is now in process of construction at Elswick, and will be delivered a few months hence. It will weigh 110 tons and have a carriage of 90 tons, the total weight of 200 tons being considerably in excess of previous undertakings. The gun will be a breech-loader and have a bore of 16 inches. Its length will be 43 feet 8 inches, but its extreme diameter at the breech will be only 5 feet 6 inches, and it will have a very elongated chase or barrel tapering down to 28 inches, with a slight swelling at the muzzle. The carriage will run on the ordinary railway gauge, but the line leading to the proof-butts will have to be partly relaid, and the bridge over the canal will probably be strengthened. After the gun has been proved at Woolwich it will be taken to Shoeburyness for the purpose of trying its range and accuracy, and it is at present a question whether the gun barge Magog can be altered to receive it, or whether it will be necessary to provide another vessel. Three guns of this description are to be made, and they are intended for the Royal Navy.

RUSSIAN HEAVY ORDNANCE.—Very large orders for naval artillery have recently been given to the Oboukhoff Steel works, near St. Petersburg, which is virtually a government establishment, and under the control of officers of the Russian war department. The orders given by the minister of marine comprise two 12-inch guns for the ironclad Katrina II., now in course of construction at Nicolaieff; four 11-inch guns for turret frigates, nine 9-inch guns for monitors, seventeen 6-inch guns for the corvettes Rynda, Vityaz, and Bobr; six 9-pounders for the Bobr, eight 4-pounders for the Rynda and Vityaz, and two 2½-inch field pieces for other vessels. There are also large requisitions for shot and shell. All the steel for the guns is of home production, and made at the government works of Briansk, where also is produced the steel for the new ironclads Nicolaieff and Sebastopol. Steel manufacture is being very much encouraged at the present time in Russia. The small-arms factory at Sestroretsk is entirely supplied with steel made at Zlataoust, which also furnishes the 12-pounder guns used by the Russian artillery. Prince Belozensky has established large steel-works at Katel-Ivanova, and has obtained from the government an order for steel rails which will keep his works employed for several years to come.—*Iron*.

FLOATING HOUSE FOR TORPEDO EXPERIMENTS.—A floating house for torpedo trials is being built at the Royal Arsenal, Woolwich, by the Laboratory Department. It is intended for use at Chatham Dockyard, where the Lords of the Admiralty have placed one

of the largest basins in the kingdom at the disposal of the War Department for the purposes of these experiments, stipulating only that no building is to be erected, and that possession must be given up at any time without notice. For these reasons a floating house has become necessary. It will contain the engine for charging the torpedoes with compressed air, the tube for firing them under water, and the other apparatus requisite for a torpedo range. For some time past the canal at the Royal Arsenal, Woolwich, which was the only piece of still water available for testing and proving the newly constructed torpedoes, has been found inadequate as regards length and depth, and attempts have been made to try the torpedoes in the Thames, but not with any great success.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

BULLETINS of the United States Geological Survey:

No. 2.—Gold and Silver Conversion Tables; a pamphlet of 8 pages.

No. 3.—Fossil Faunas of the Upper Devonian: pamphlet 36 pp.

No. 4.—On Mesozoic Fossils; pamphlet 37 pp.

No. 5.—A Dictionary of Altitudes in the United States; pamphlet 325 pp.

No. 6.—Elevations in the Dominion of Canada; pamphlet 42 pp. Washington: Government Printing Office.

Transactions of American Society of Civil Engineers. January.

Bulletin of the Philosophical Society of Washington, Vol. VII.

From Cassell & Co.: The Quiver, The Family Magazine, and The Magazine of Art; the latter embellished with five full-page illustrations.

Selected Papers of the Institution of Civil Engineers:

The General Theory of Thermo-Dynamics. By Professor Osborne Reynolds, F. R. S.

No. 1,992.—The Art of Making Paper by the Machine. By James William Wyatt, Assoc. M. Inst. C. E.

No. 1,999.—On Hauling out and Launching Vessels Sideways. By Murray Jackson.

No. 2,016.—Pumping Machinery for Draining Marshes. By Thomas Richard Guppy, M. Inst. C. E.

No. 2,022.—Removal of Buddonness Lighthouse. By David Cunningham, M. Inst. C. E.

No. 2,023.—Notes on Compressed Air. By John Kraft, M. Inst. C. E.

No. 2,025.—The Burnham Sewage Outfall Works. By Alfred Barton Brady, Asso. M. I. C. E.

No. 2,029.—Electric Lighting for Steamships. By Andrew Jamieson, F. R. S. E.

No. 2,036.—The Barmouth Waterworks. By Thomas Roberts, Assoc. M. Inst. C. E.

THE CIVIL ENGINEER'S POCKET-BOOK. By JOHN C. TRAUTWINE, C. E. Revised, corrected and enlarged by John C. Trautwine, Jr., C. E. New York: John Wiley & Sons.

A book so widely known as this needs no comment, certainly no praise. Many engineers regard it as the only necessary cyclopedia of useful knowledge, and many of the profession who began to use the first edition fourteen years ago are probably now suffering from impaired or lost eyesight from too assiduously laboring over the painfully small type which was necessarily employed in condensing an encyclopedia into a pocket-book.

The revisions and corrections in the new edition are numerous. It requires five pages of the preface to specify them. Many omitted paragraphs have not been replaced, so that a large number of scars are apparent when turning over the leaves.

The additions to the work are an undoubted improvement.

ANNUAL REPORT OF THE STATE GEOLOGIST OF NEW JERSEY FOR THE YEAR 1884. By GEO. H. COOK, LL. D. Trenton: State Printer.

Superintendent Cook's Reports are always valuable to the people of the State at least, and always contain something of interest for students of geology everywhere.

Considerable space is given to the description of the columnar trap rocks at Orange, in which there was a widespread popular interest manifested last Fall.

The chapters on the Drainage of the Great Meadows, and on the Purifying of Water, possess a far more than local interest.

The maps and other illustrations are excellent.

HOW TO DRAIN A HOUSE. By GEO. E. WARING, Jr., M. Inst. C. E. New York: Henry Holt & Co.

This little book of 222 pages presents the subject of House Drainage in 26 chapters. It is a compact manual for the householder. Many who would refuse to read the more complete treatises on this important subject will probably be attracted by the exceedingly concise form in which the leading principles are presented.

The few illustrations are pretty good.

PHOTOGRAPHY FOR AMATEURS; A NON-TECHNICAL MANUAL FOR THE USE OF ALL. By T. C. HEPWORTH. Cassell & Co. 1884.

This little volume contains more information valuable to the amateur than many of the larger and more pretentious publications. The chapter on portraiture is valuable as giving directions for taking properly-lighted pictures in ordinary dwelling rooms—a feat which few amateurs accomplish successfully. Pages 29 and 30, on lenses, are worthy the perusal of anyone intending to buy an outfit, and may save the purchaser many dollars. On p. 72 a negative washing-box is given, which most workers in photography will find unpractical. In the chapter on development an improvement could be made by using the same terms when speaking of fluids, and not employing "oz." in one place and "pints" in another. Chapter 11 contains some good hints on mounting prints, including the making of starch paste and mounting with gelatine in alcohol, a method which will not cockle the mounts. A good index is another recommendation for this little volume.

TOPOGRAPHICAL SURVEYING. VAN NOSTRAND'S Science Series, No. 72. Price 50 cents.

This work contains four papers on the following topics, viz.: "Topographical Surveying," by Geo. J. Specht, C. E.; "New Methods in Topographical Surveying," by Professor A. S. Hardy; "Geometry of Position applied to Surveying," by John B. McMaster, C. E.; "Co-ordinate Surveying," by Henry F. Walling, C. E. The Art of Surveying, now but in its infancy in this country, and with surroundings adapted to afford it a huge growth, finds in these short essays elements tending to promote a rapid and healthy development, a boon which it cannot be said to have enjoyed at the hands of the American surveyor, because, perhaps, necessity has not seemed to demand it. Now that potent reasons for determining the position and configuration of our lands are arising, it is a source of satisfaction to see that scientific literati are giving this important subject the attention it deserves.

The paper on Topographical Surveying treats on the use of the stadia. The essential details of field practice and the manner of keeping and plotting field notes are given in a very complete manner. The paper closes with a short chapter on the use of the slide rule to facilitate the computation of the field notes, the usefulness of which as a labor-saving machine is apparent.

Mr. Hardy's description of photographic surveying is very interesting, and undoubtedly will inform a large body of our surveyors, for the first time, of the possibility of locating, by intersections, an almost infinite number of points by but two observations. The office work consists in finding the intersection of the two cones determined in the field, a simple geometrical operation.

The paper on the Geometry of Position applied to surveying is very instructive, and every surveyor who aims to become accomplished in field work should number this method among his resources.

The paper on Co-ordinate Surveying presents a plan for conducting land surveys by referring them to stations, the co-ordinates of which have been determined by a complete system of trigonometrical surveys, with references to some established point. The objects to be attained are an accurate determination of the positions of corners and directions of boundaries, and the avoiding in a large measure of land litigation. A review of what has been accomplished in this direction, the faults of the present method, or rather lack of method, of conducting our surveys, is given in a series of short chapters as an introduction to the writer's plan. The captions of two of these chapters speak volumes to those who have had opportunities of becoming familiar with how our land surveys are made. They are "Faulty descriptions in land conveyances," and "Imperfections of the surveyor's compass." The method of procedure in carrying on the different operations relating to this plan and two examples of its application are presented. The manner in which the subject is handled is very clear, and the demonstration of the principles require no more mathematical knowledge than what every surveyor should be expected and required to possess. Apropos of this question it may only be necessary to refer incidentally to the circumstance, quite widely known, we presume, that one of the results of the trigonometrical survey of New York State was to establish the fact that most remarkable errors existed in the location of towns and boundaries of counties as laid down on the then current maps. It is to be hoped that this paper will be extensively read by those interested in this subject, and hasten the millennium of bringing order out of the present chaos in our land surveys.—*Abstract of Review in Mechanical Engineer.*

MISCELLANEOUS.

THE "NOVELTIES" EXHIBITION.—The announcement of the Managers of the Franklin Institute of their intention to hold another exhibition in Philadelphia, during the autumn of the present year, will be news of interest to all our readers. The Electrical Exhibition of last year was an unqualified success in every way, and was most creditable to this useful and venerable institution, and to all who were concerned in it.

The managers have decided this year to hold an exhibition of a more popular character. It will be known as the "Novelties" Exhibition, and will be devoted, as its title indicates, to the display of such recent inventions, improvements and discoveries in the arts and manufactures as may be deemed worthy of admission.

It will afford our inventors and manufacturers an excellent opportunity of bringing to public notice their best and most recent achievements. That the character of the forthcoming event will undoubtedly be maintained up to the highest standard of excellence, goes without saying, and our people may look forward to the "Novelties" as a rare source of interest and instruction.

From the announcement of the managers, the "Novelties" Exhibition will be opened on Tuesday, September 15th, and will be closed on Saturday, October 31st.

CEMENTS FOR SPECIAL PURPOSES.—The value of a cement is, first, that it should become a strongly cohering medium between the substances joined; and, second, that it should withstand the action of heat, or any solvent action of water or acids. Cement often fails in regard to the last consideration. For waterproof uses several mixtures are recommended, and the following may be mentioned: One is to mix white lead, red lead, and boiled oil, together with good size, to the consistency of putty. Another is powdered resin, 1 oz., dissolved in 10 oz. of strong ammonia; gelatine, 5 parts, solution of acid chromate of lime, 1 part. Exposing the article to sunlight is useful for some purposes. A waterproof paste cement is said to be made by adding to hot starch paste half its weight of turpentine and a small piece of alum. As a cement lining for cisterns, powdered brick 2, quicklime 2, wood ashes 2, made into a paste, with boiled oil, is recommended. The following are cements for steam and water joints: Ground litharge, 10 lbs., plaster of

Paris, 4 lbs., yellow ochre, ½ lb., red lead 2 lbs., hemp, cut into ½-in lengths, ½ oz., mixed with boiled linseed oil to the consistency of putty. White lead, 10 parts, black oxide of manganese, 3, litharge, 1; mixed with boiled linseed oil. A cement for joints to resist great heat is made thus: Asbestos powder, made into a thick paste, with liquid silicate of soda. For coating acid troughs, a mixture of 1 part pitch, 1 part resin, and 1 part plaster of Paris is melted, and is said to be a good cement coating.

Correspondents frequently ask for a good cement for fixing iron bars into stone in lieu of lead, and nothing better is known than a compound of equal parts of sulphur and pitch. A good cement for stoves and ranges is made of fire-clay with a solution of silicate of soda. A glue to resist damp can be prepared with boiled linseed oil and ordinary glue; or by melting 1 lb. of glue in two quarts of skimmed milk, shellac, 4 oz., borax, 1 oz., boiled in a little water, and concentrated by heat to paste. A cement to resist white heat may be usefully mentioned here. Pulverized clay, 4 parts; plumbago, 2; iron filing, free from oxide, 2; peroxide of manganese, 1; borax, ¼; seasalt, ¼; mix with water to thick paste, use immediately, and heat gradually to a nearly white heat. Many of the cements used which are exposed to great heat fail from the expansion of one or more ingredients in them, and an unequal stress is produced; or the two substances united have unequal rates of expansibility or contractility; the chemical or galvanic action is important. The whole subject of cements has not received the attention it deserves from practical men. Only Portland cement has received anything like scientific notice, and a few experiments upon waterproof, heat-resisting, and other cements would show which cements are the best to use under certain circumstances.

Assyrian Science.—Last Wednesday afternoon Mr. W. St. Chad Boscawen delivered one of a series of weekly lectures at the British Museum on "Assyria;" the attendance was large. In the course of his address the speaker drew attention to the amount of scientific knowledge possessed by the dwellers in the city of Nimrod, mentioned in the opening chapters of the Book of Genesis, and he stated that the present knowledge of the manners and customs of the people who dwelt in those cities during a period ranging from 2000 to 4000 years B. C. is considerable, even to the details of the lives of certain private persons down to their very family squabbles. As to their scientific knowledge, they had made some progress in mensuration, and they laid down plans of buildings "to scale;" their standard of measurement was the Babylonian cubit, which they subdivided into a number of equal sections. They had a table of square and cube roots, they calculated by the scale of 60, and they divided the circle into 360 parts. In the earlier periods their sculpture, in which they were advanced, was more true to nature than in the later periods, when it became conventional. He should like to know what the British workman would think of the material they carved, for it was porphyry so hard that it would turn the edges of the best chisels; in all probability they used the diamond drill, as the Egyptians certainly did, and spent unlimited time over their work out of fealty to their king or reverence for their God. They worked bronze with the hammer, and they cast statues with the same alloy; one of their earliest hymns speaks of a good man shining like brass cast out of a crucible. In music, they had the harp, pipes, and cymbal. They knew the colors assumed by light; Mars was described to them as a red orb, and Mercury as a blue one; in one of their hymns a scribe speaks of the sky being as blue as the sea. They had words for the compound as well as the primary colors, including names for reddish-brown, purple and gray. Blue and purple were connected in their minds with the idea of darkness, and the same appears to have been the case with the ancient Greeks; he was not quite sure that such was the case with the latter people. The Euphrates Valley, in fact, was the cradle of civilization; upon its banks were city kingdoms, each city having its own king, one of whose duties it was to sit at the gate of the city to give judgment. Old commentators had found various sites for the four cities of Nimrod, varying in position from Ireland to the banks of the Ganges; but modern explorations had solved the mystery. The true site of Babylon was the only one which had been preserved by tradition; the literal interpretation of the word "Babylon" was "The Gate of God."

A paper on some irregularities in the values of the mean density of the earth, as determined by Baily, was read on the 26th ult. by Mr. W. M. Hicks. The author showed that the numbers obtained by Baily for the mean density of the earth depended on the temperature of the air at which the different observations were made; and he exhibited a table showing that as the temperature increased from 40 deg. Fah. to 60 deg. Fah. the deduced mean density fell continuously from 5.734 to 5.582. He considered several possible causes of error, but showed that they were either inadequate to explain the irregularities, or tended in the opposite direction. The only further suggestion that occurred to him was that Baily's personal equation was a function of the temperature, leading him, as his temperature rose, to estimate distances more liberally.

At a recent meeting of the Cambridge Philosophical Society, a paper was read by Mr. A. H. Leahy on the pulsation of spheres in an elastic medium. The problem of two pulsating spheres in an incompressible fluid has been discussed by several writers. The author considers the analogous problem in the case in which the medium surrounding the spheres has the properties of an elastic solid. He finds that the most important term in the expression of the law of force between the two spheres varies inversely as the square of the distance between them. This force will be an attraction if the spheres be in unlike phases, a repulsion if they be in like phases at any instant. The next term in the expression varies inversely as the cube of the distance between the two spheres, and is always a repulsion.